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GEOPHYSICAL EXPLORATION OF THE OCEAN BOTTOM

SYMPOSIUM ARRANGED BY THE AMERICAN GEOPHYSICAL UNION

HELD AT

THE AMERICAN PHILOSOPHICAL SOCIETY

NOVEMBER 26, 1937

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RICHARD M. FIELD, Chairman, Committee on Geophysical and Geological Study of Ocean Basins, American Geodetic and Geophysical Union.

New Methods of Marine Surveying 9
G. T. RUDÉ, Chief, Division of Hydrography and Topography, United States Coast and Geodetic Survey.

Present Status of the Problem of Submarine Canyons 27
H. C. STETSON, Research Associate in Submarine Geology, Woods Hole Oceanographic Institution.

The Technique of Securing Undisturbed Core-Samples of the Ocean Bottom 35
CHARLES SNOWDEN PIGGOT, Geophysical Laboratory, Carnegie Institution of Washington.

The Geological Survey's Work on the Piggot North Atlantic Deep Sea Cores 41
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THE IMPORTANCE OF GEOPHYSICS TO SUBMARINE GEOLOGY

RICHARD M. FIELD

Chairman, Committee on Geophysical and Geological Study of Ocean Basins. A.G.G.U.

BECAUSE of the magnitude of the subject and the large number of papers, for the time allowed, each speaker can hardly hope to do more than outline the essential matter within his potential field. I shall, therefore, make my remarks as brief as possible; with no desire to "beg the argument" or to present conclusions previous to the evidence from which they may be properly deduced.

Submarine geology, or the exploration of the submarine and suboceanic lithosphere, is not properly a subdivision of oceanography, except in so far as it makes use of the experience of individuals and the equipment of institutions engaged in the physical, chemical, and biological study of marine and oceanic waters. Submarine geology differs only from terrestrial geology by the methods employed in mapping and sampling a major portion of the surface of the earth which is covered with water, from a few feet to several miles in depth. Until quite recently, the geologist has had neither the opportunity nor the machinery for exploring the topography, sedimentation, lithology, and structure of the suboceanic lithosphere. In fact, he was worse off than the astronomer who, even if he can not collect specimens of the moon, can at least photograph and map the major features of a complete sample (one-half) of its entire surface. This inability to acquire even fundamental physiographic data for the great suboceanic areas has unquestionably led geologists to consider them relatively negligible or *negative* as compared with the continents, whose stratigraphical and structural evolution is

becoming increasingly well known. This *influence of ignorance* is clearly expressed in such opposed major geological hypotheses as "the permanency of ocean basins" and "continental drift," both of which, however, include the postulate that the ocean basins never were dry land. This postulate of the permanency of ocean basins is all the more intriguing, for, since the suboceanic lithosphere must have been affected by deformative processes, topographic maps of significant areas will allow the geologist to study the contours of strongly pronounced folds and scarps which, as they developed, have not been subjected to erosion. On the other hand, if such a significant area as the middle Atlantic ridge is proved to have a distinctly subaerial topography, such information will tend to weaken the postulate of the permanency of the Atlantic ocean basin. For several reasons, it would be interesting to compare and contrast the physiographies of the Subatlantic and Subpacific lithosphere. To put it still more plainly, any accurate topographic data regarding the subocean lithosphere are bound to be of great geologic importance.

It is interesting to note that the theory of isostasy is particularly significant for two reasons. First, because of geological and geophysical evidence as to the structural evolution of continents, and, second, because of geophysical data concerning both continents and ocean basins. Thus a peculiar importance of the theory of isostasy is that it is the outstanding attempt to explain the origin and evolution of the major structural features of the lithosphere in terms of coördinated geological and geophysical data. Many geologists have quite properly claimed that many of the structural features of continents, especially great folds and overthrusts can not be either directly or indirectly due to isostasy, and, therefore, it is not an all-inclusive factor or complete *modus operandi* for explaining diastrophism, including the origin of mountains. On the other hand, the support obtained for isostasy by the coördination of gravity data and geological observations has quite recently spurred a few geologists to include gravimetric and other geophysical techniques in their

surveys for fundamental structures, both continental and suboceanic, with remarkably interesting results.

The keynote of this symposium on suboceanic geology is geophysics. This is true for two reasons. First, because geophysical methods are the best for mapping the topography of the ocean bottom, and, second, because only geophysical methods may be employed to determine the composition and structure of the suboceanic lithosphere. What, therefore, is geophysics?[1] According to Dr. G. Angenheister, Director of the Geophysical Institute of the University of Göttingen, the word "geophysik" appeared in Meyer's *Conversatsionslexicon*, published in 1853. The term must have been in use sometime before that. Both A. Mühry (1863) and K. Löppritz, Sr., aided in procuring its general acceptance. Among the first comprehensive treatises in the English language on the subject is *The Physics of the Crust*, by G. Osmund Fisher, published in 1861. The scope of geophysics is broad and difficult to define, especially in relation to geology. It is important to note that geophysics is not necessarily synonymous with physical chemistry, and, therefore, with geochemistry, although both are intimately related to geology. In the field of geophysics, the many methods and techniques are classified as either acoustic, gravimetric, seismic, magnetic, electric, geothermal, or radioactive. All of these methods have been applied to the study of the structure of continents. More recently several of these methods have been applied to the study of the suboceanic crust of the earth, and all of them may soon be as effectively and accurately controlled from a ship or from a submarine as they now are from the dry and stable platform of the land.

During recent years geophysical techniques have been highly developed, particularly in relation to prospecting for petroleum through the mapping of subsurface geological structures, the traces of which may, or may not, be exposed at the surface of the earth. With the possible exception of atomic and astronomical physics and certain phases of

[1] Geophysics. Encyclopedia of Science, Van Nostrand, 1938.

meteorology, the greatest field of geophysics has been the development of classical physics as applied to problems in geodesy and structural geology, both fundamental and economic. Today, geophysics bridges the gap between physics and geology. Thus geophysics is not a "borderline science" because its methods and derived data are properly interpreted only in terms of structural geology. Geophysical methods, therefore, are among the many, including those of geochemistry, paleontology, mineralogy, petrology, etc., which are available to the geologist in his study of earth processes and earth history.

The geologist has discovered that the history of the earth is expressed not only in the present irregularities of its surface, but also in the rocks out of which the present surface has been carved by the several processes of erosion. While many of these rocks were originally formed at or near the surface of the earth, some of them show evidence of having been subsequently buried to a depth of ten to twelve miles before they were again exposed at the surface. Thus the geologist is reasonably sure that he has available at the surface of the earth samples of practically all types of rocks which have been formed throughout the *geological* history of the earth. This great history of the conflict between processes of diastrophism, its accompanying vulcanism, and the processes of denudation is written in terms of structural geology, as disclosed by the arts of the stratigrapher and structural geologist. The geologist and geophysicist in their studies of continents have discovered that by working together they can extend both the depth and area of their structural survey of the crust of the earth, and thus greatly add to our knowledge of its history.

Thus the increasing success of geophysical methods is largely, if not entirely, due to the mutual education and close coöperation of geologists and geophysicists. The primary responsibility of the geologists has been to determine the areas to be surveyed and the manner of their surveying, and to help the geophysicists interpret their data in terms not

only of petrology but rather of structural geology. The duty of the geophysicists has been to adapt and develop the application of experimental physics to the geophysical exploration of the earth's crust, and to suggest to the structural geologist new methods and techniques for extending the area as well as the depth of his surveys. During the development of this geophysical-geological coöperation, there naturally and properly arises, from time to time, considerable debate as to the relative values of the purely geophysical and purely geological data. This debate has not only greatly advanced our knowledge of the structural geology of continents, but has also expanded the field of geophysics.

In considering the exploration of the suboceanic crust, we must take into account the special features of the problem: First, in terms of the relatively small amount of available physiographical and geological data concerning the suboceanic as compared with the continental crust; second, in terms of existing geological hypotheses and theories advanced to explain the origin of continents and ocean basins; and, third, in reference to the present and future possibilities of geophysical methods of obtaining accurate data concerning the physiography and structure of the 70 to 80 per cent of the crust of the earth which is covered by water and ice. The outstanding geological problem is the permanency of oceanic basins, and it is doubtful if this problem can be solved by the study of the continents and oceanic islands alone.

Many, if not most, of the major continental structures border the continents, and most continental structures certainly continue beyond the strand line, perhaps even beyond the continental margin. For example, we now know that the structure of the Appalachians not only suggests that the thrusting came from the oceanic area, but also that we can see only the western side of what was probably a more symmetrical and widespread orogeny which was not terminated at the continental margin unless it has been torn away from complementary, transatlantic structures. This suggestion, affected as it is by the lessons learned from the long

study of the Alpine and Scottish-Scandinavian geosynclines, is diametrically opposed to the earlier and simpler concept of the origin of the Appalachian geosyncline and its associated geanticline, Appalachia. Regardless of any, or all, geological theories concerning the origin of ocean basins, we should attempt to trace known continental structures across the submerged continental margins and into the suboceanic lithosphere. This is particularly true for the Atlantic Ocean basin, the stratigraphic and structural matching of whose continental margins has been the chief stimulus to the several hypotheses which attempt to explain the major pattern of the face of the earth. What we particularly need are physiographic and structural data concerning the Atlantic, Pacific, Indian, and Arctic suboceanic crust. Can geophysical methods help us to acquire these data: First, by penetrating the oceanic waters which cover more than two-thirds of the surface of the earth; second, by providing accurate topographic maps of the suboceanic crust; third, by providing us with samples of the sediments wherever they may occur on the surface of the topography; and, fourth, and most important of all, help us to carry our structural studies deep beneath the ocean bottom, with the same degree of accuracy which is now obtained in the geophysical-structural exploration of the continents. This question is the primary object of this symposium and the following roundtable conference.

In closing, I should like to call to your attention a phase of the geophysical exploration of the ocean bottom which is of considerable political and human interest, namely, that the major portion of the earth's crust constitutes a vast "no man's land," as yet practically unexplored and either policed or fought over by hundreds of naval vessels which plow the waves from one to several miles above its surface. Many of these naval vessels, including submarines and mine sweepers, are admirably adapted to the geophysical exploration of the suboceanic lithosphere. Indeed, without such naval assistance, we can not hope to execute a detailed and comprehensive survey of even a significant portion of the suboceanic litho-

sphere with reasonable economy and efficiency. Following the precedent which has already been set up by the individual efforts of the Dutch, British, German, French, Italian, and American navies in relation to sonic gravitational and magnetic surveys, is this not an excellent time to suggest that the complete geophysical exploration of the ocean bottom is of common scientific interest to all nations even if they never can hope to take possession of it, any more than the astronomers are able to colonize the planets or their satellites.

NEW METHODS OF MARINE SURVEYING

CAPTAIN G. T. RUDÉ

Chief, Division of Hydrography and Topography
U. S. Coast and Geodetic Survey

It gives me pleasure to accept the invitation of your society to describe briefly the methods used for the offshore hydrographic surveys which the Coast and Geodetic Survey has been making in recent years. The officers of the Coast and Geodetic Survey rightly consider the hydrographic surveys conducted for making nautical charts as the routine and immediately practical work of the Bureau and regard the nautical charts themselves as paramount to the safety of life and property at sea. It is fortunate, however, that these surveys, required for a utilitarian purpose, do have scientific value in themselves in addition to the contributions which come from the development and use of scientific methods in the work, which from time to time bring unexpected additions to scientific knowledge. The areas dealt with embrace in general the continental platforms, submerged mountains, and submarine valleys. Although the regions include only a small portion of the great ocean basins, they do represent a relatively large share of the oceanographic problems.

The deep ocean basins now appear to have had a similar geologic history to the surfaces of the continents above the sea. The beginning of the period of enlightenment, I believe, can be attributed to the application of sonic methods to hydrographic surveys. The development of echo sounding, for instance, has been not only a great aid to navigation, but it has in turn made it possible to survey many hundreds of square miles in less time than required to take a few scattered deep soundings by old methods in use when I joined the Survey thirty-five years ago. The other important factor is

the development of radio acoustic ranging for controlling hydrographic surveys well out to sea.

At sea beyond the range of visual positions which can be obtained from sextant angles on shore triangulation stations, positions of the survey ship have heretofore been subject to considerable uncertainty, this uncertainty naturally increasing with the distance beyond the limits of the shore control. Various methods were attempted and utilized for controlling hydrographic surveys out of sight of land, but none of these gave satisfactory accuracy of position until radio acoustic ranging was developed. Soundings made by wire and sinker are quite accurate as to depth when taken under good conditions of weather and sea, but the uncertainty of the actual offshore positions until recent years often led to erroneous deductions from the soundings. For example, the few soundings made in the vicinity of the Hudson Submarine Valley prior to the work of the past year (1936) indicated an S-shaped outer valley. This was due in part to the inaccuracies of position in some of the earlier soundings and also to the scarcity of soundings in the region. The work of 1936 has proved that the few deep soundings from which the erroneous inference was made were taken in an entirely different submarine valley to the northeast of the main Hudson valley. Many other examples could be cited.

Echo sounding instruments now have been developed to such a degree that practically a continuous accurate profile of the sea bottom under the surveying vessel's course can be obtained. The number of soundings to be recorded depends almost wholly upon the facility with which they may be written down in the record. Some forms of echo sounding instruments actually record the profile of the vessel's course on a graph. It is interesting to contrast this modern ease of sounding with that of the Challenger Expedition, which on the notable cruise between the years of 1873 and 1876 made a total of 504 deep-sea soundings, or with the record of the *Blake* between 1874 and 1879 when the then remarkable number of 3,195 deep-sea soundings was achieved. Now in

contrast, in fifteen working days in the month of July 1937, the party on the Coast and Geodetic Survey Ship *Lydonia* recorded 12,498 soundings on the continental slope and covered an area of 1,764 square statute miles extending more than 150 miles offshore, all of which was precisely controlled by radio acoustic ranging and taut wire traverses tied in to the shore triangulation net on North American 1927 Datum. This included the time required for two trips to port for supplies.

During five months in the summer of 1937 the party on the Coast and Geodetic Survey Ship *Hydrographer* covered an area in the Gulf of Mexico of 6,513 square miles off the Texas coast, recording 82,986 soundings.

Similar results have been accomplished on the Pacific coasts of the United States and Alaska. Within the past six years, 285,000 square statute miles of marine surveys have been made of the coastal waters of the United States and possessions. This is no inconsiderable portion of the earth's surface. While the greatest contribution to science represented by these surveys probably is the physiographic detail mapped in the course of the surveys, there are other results valuable to science. Bottom specimens have been taken and temperature and salinity observations have been made. The determination of the characteristics of the ray paths of sound waves in sea water, resulting from the experiments made in the Santa Barbara Basin off the coast of Southern California in 1935 and 1936 by the Coast and Geodetic Survey Ship *Pioneer*, is a contribution of fundamental importance. These experiments were made primarily to establish the soundness of our ray-path theories for further increasing the accuracy of radio acoustic ranging and to learn something of the anomalies to be encountered in practical sonic work utilizing the velocity of sound in sea water. Briefly, the results of these experiments confirmed the relation between the apparent horizontal velocity of sound and the theoretical velocity corrected for bottom temperatures and salinities in shoal water. This relation has been known for some time, and

although empirical it has provided a very convenient method for use in practical radio-acoustic ranging. It was definitely established that the sound travels in a series of reflections and refractions, with some diffraction for short distances, and that the path is materially affected by the even or uneven character of the bottom or steep slopes near the receiving hydrophone.

It was found that the apparent horizontal velocity of sound in the open sea is a discontinuous function of the distance, the magnitude of the discontinuities depending on the bottom profile, depth and size of the bombs, and sensitivity of the hydrophone. With decreasing depth of bottom, or increasing bomb-size, the magnitude of the discontinuities decreases and may become negligible under certain conditions. Many other interesting and valuable facts were disclosed by these experiments, a study of which is now being made and which it is believed will materially add to the accuracy and efficiency of radio-acoustic ranging methods. The British Admiralty Tables of theoretical velocities of sound in water were verified within two-tenths of one per cent. With these valuable tables confirmed by actual comprehensive tests, one can see that the velocity of sound anywhere in the ocean may be obtained by temperature and salinity observations, and this makes easily available a length or base for any desired explorations of limited areas at sea.

Another important contribution comes from the discovery of numerous submarine valleys indenting the edges of the continental shelves and the establishment of the apparent stream-erosion nature of the valleys. A few of the more prominent ones are illustrated in the accompanying figures. The discovery of well dissected submarine regions indicates the desirability of further exploration of some of these features in other parts of the earth by the methods now at our disposal.

A better conception of the nature of these surveys and the practicability of application to studies of the ocean basins can be had when the methods are understood. Echo sounding is almost self-explanatory. A description of the development

of the various forms of commercial instruments for this purpose is hardly necessary. The instruments so far used by the Survey have been commercial forms of the Fathometer, slightly modified by our own engineers to increase the accu-

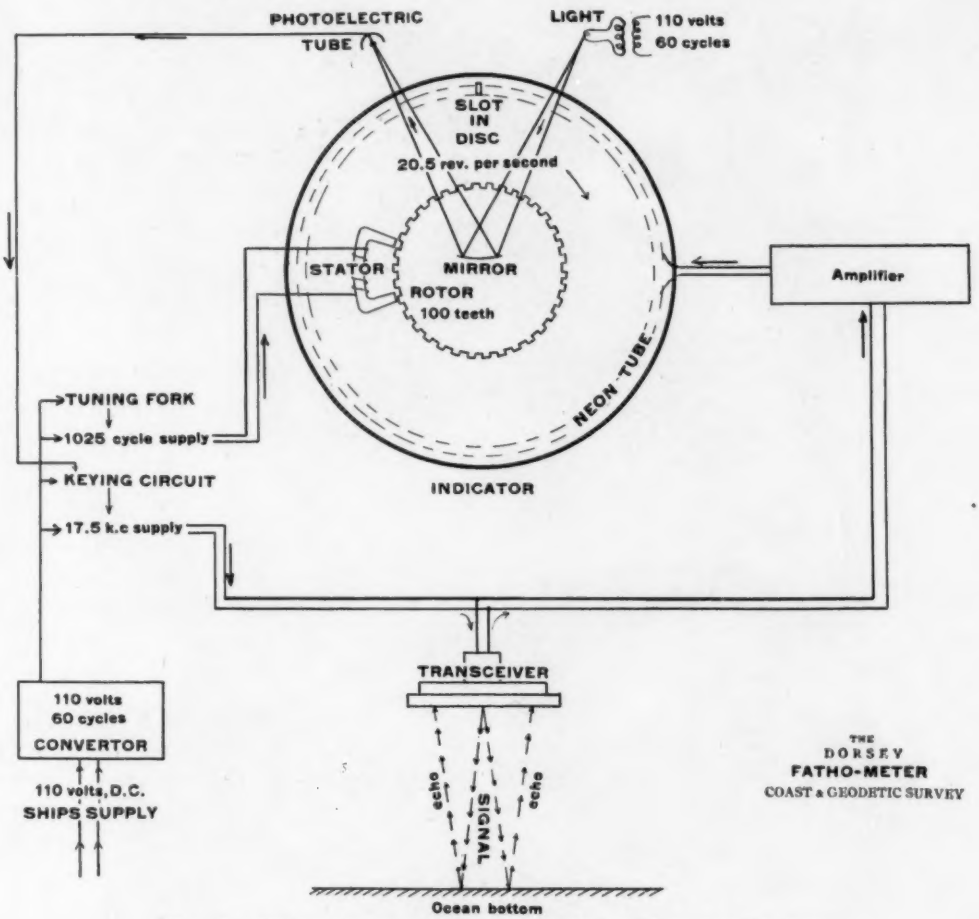


FIG. 1 Schematic representation of Dorsey Fathometer No. 1.

racy, except the Dorsey Fathometers which were developed in our laboratory. Essentially the Fathometer indicates depths by measuring the time required for a sound impulse to travel to the sea bottom and for the echo to return. This usually is done by a flashing light on a rotating disc behind a

graduated dial. The accuracy of the instrument depends upon the constancy of speed of rotation of the disc; and to insure the accuracy desired, a synchronous motor controlled

FIG. 2. Dorsey Fathometer No. 1, front view.

by a tuning fork is used in the Dorsey Fathometers. The sketch in figure 1 illustrates the general arrangement and operation of the Fathometer and figure 2 shows the appearance of the indicator.

The method of radio acoustic ranging (called R.A.R. for brevity) has given a satisfactory and accurate means for position determination at long distances from shore. It is llustrated graphically in figure 3. Two or more radio stations on shore are automatically keyed by subaqueous impulses received by the hydrophones anchored at some distance from shore. The surveying vessel, using small bombs of TNT, proceeds along the necessary courses sounding with fathometer. When a position is necessary a bomb is dropped overboard. The time of explosion is recorded on a chronograph tape on the ship. The sound travels through the water and when the impulse reaches the hydrophones inshore, radio impulses are sent to the surveying vessel and recorded upon the same chronograph tape as was the original explosion. The time interval thus measured, multiplied by the apparent horizontal velocity of sound in the water gives the distances of the surveying ship from the several hydrophones. The positions of the hydrophones are always determined with reference to the triangulation on shore. On the Atlantic and Gulf coastal shelves it has been necessary to place the hydrophones well out from shore in order to avoid the serious attenuation of the sound experienced in very shoal water. For this purpose it has been the practice, until the last year, to use station vessels. The development of the sono-radio buoy has been successful and figure 4 illustrates the method of R.A.R. with sono-radio buoys replacing the station ships. The sono-radio buoys have been used up to sixty miles or more.

The taut wire apparatus is a device developed by the British. It is a large reel of fine wire (usually over 140 nautical miles) which can be payed out under uniform tension over a registering wheel or sheave as the surveying vessel steams along, thus enabling the surveyors to measure distances accurately in water of relatively shoal depths and over uniform bottom.

The sketch in figure 5 illustrates the correlation of the various methods used in making a survey over such an

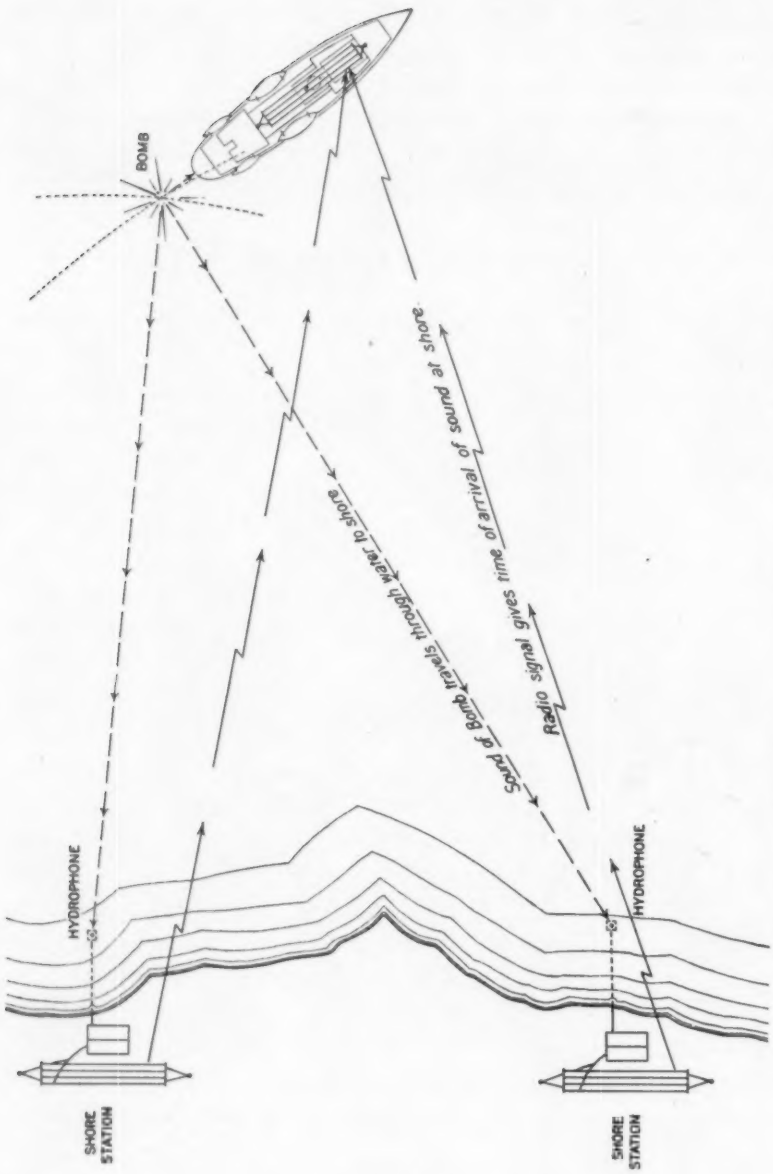


FIG. 3. Finding position by radio acoustic ranging. This diagram illustrates use with shore stations.

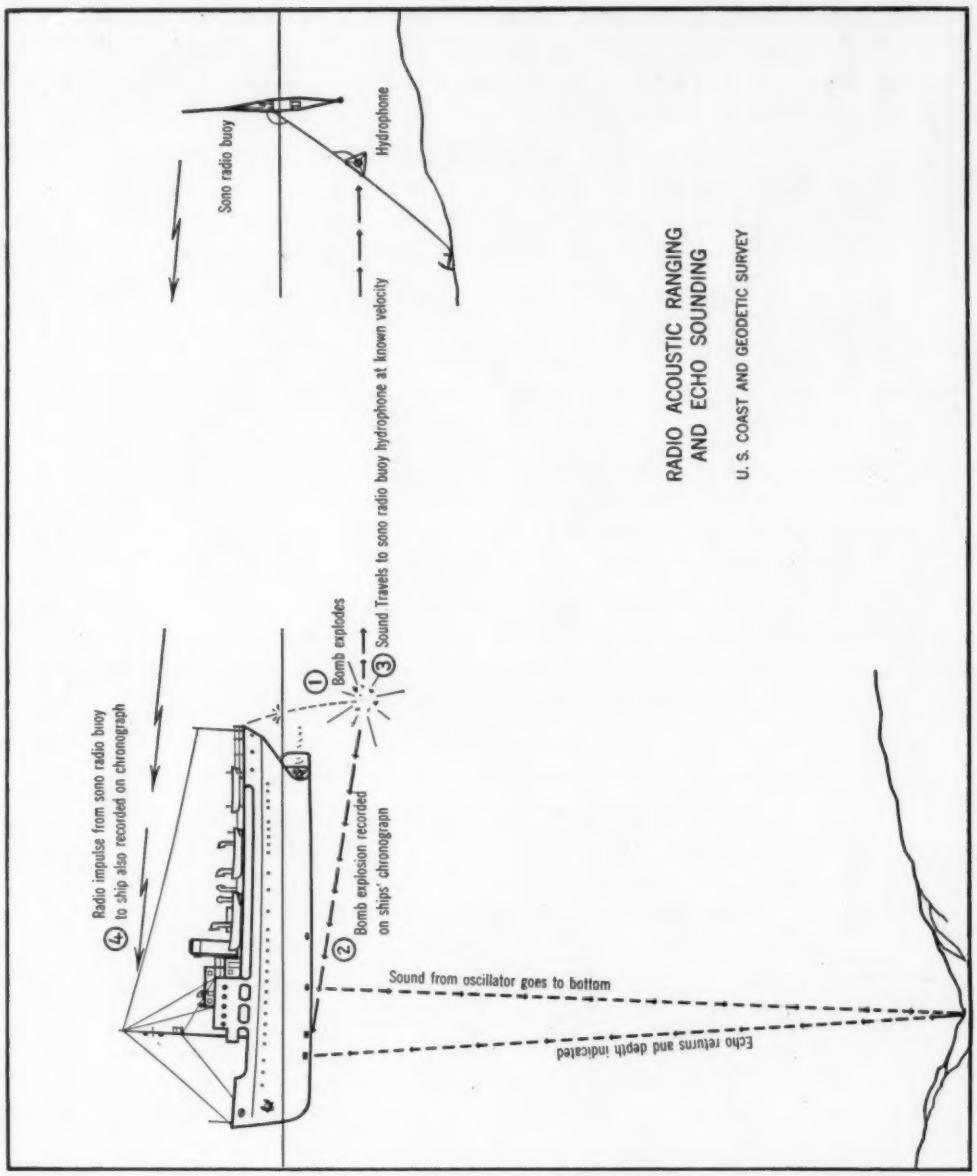

RADIO ACOUSTIC RANGING
AND ECHO SOUNDING
U. S. COAST AND GEODETIC SURVEY
Sono radio buoy
Hydrophone
Sound Travels to sono radio buoy hydrophone at known velocity
Bomb explodes
Bomb explosion recorded on ships' chronograph
Radio impulse from sono radio buoy to ship also recorded on chronograph
Sound from oscillator goes to bottom
Echo returns and depth indicated

FIG. 4. Radio acoustic ranging and echo sounding. This diagram illustrates use of sono-radio buoys.

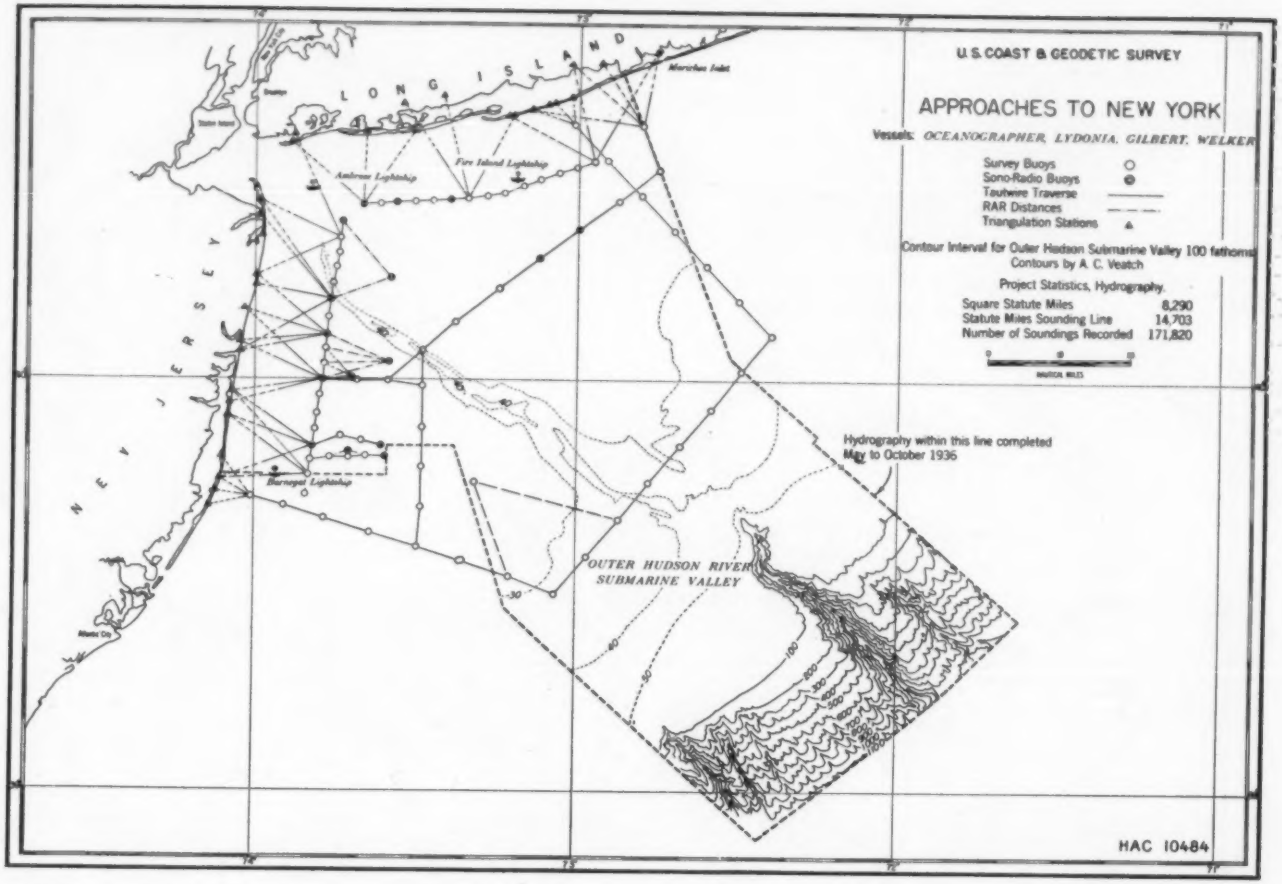


FIG. 5. Survey of approaches to New York, 1936. Illustrates the use of various methods, and shows general configuration of Hudson Submarine Valley.

extensive area. The small circles are positions of survey buoys which have been anchored and located by the taut wire apparatus. They are used as reference stations by the surveying vessel as the sounding is carried out. When the buoys have been anchored approximately in the desired positions, as determined by courses and log distances, the vessel starts at the inshore end of the row of buoys, for instance at the position near Fire Island. After anchoring one end of the fine steel wire, the vessel's position is determined by sextant angles measured between triangulation signals on shore, and a run is made along the line of survey buoys, paying out the wire under constant tension. The distances between buoys are read from the counter on the sheave as the buoys are passed. The directions between the buoys are determined by measuring simultaneously a vertical angle between the horizon and the sun, and an inclined angle between the sun and a pair of the buoys when the latter are on range. This can be done as the vessel steams along full speed and a line of echo soundings also obtained. In the above case (Fig. 5) the traverse thus run, extending southeastward 50 miles from Fire Island, thence southwestward 57 miles, thence west-northwestward to the vicinity of Barnegat Lightship had a closing error of 2¼ feet per mile. The total length of the traverse was 155 statute miles. Supplemental traverses were run where necessary to provide control for the entire area. The line of buoys indicated about twelve miles off the New Jersey coast and parallel to the shore was located by frequent visual observations on shore triangulation as well as a taut wire traverse along the line of the buoys. The heavy black circles indicate points at which sono-radio buoys were used. This is the first project where the sono-radio buoys were tested for practicability under actual radio acoustic ranging. Station ships were used during the season, as had been done in previous years, so that in case of failure of the new sono-radio buoys no hydrography would be lost. Outside of the two lines of buoys parallel to the coasts about twelve miles offshore, all of the

work in this area was controlled by radio-acoustic ranging. The work of the 1937 season has demonstrated the reliability of the sono-radio buoys and it no longer will be necessary to use station ships for this purpose.

The Hudson Submarine Valley is shown roughly on this figure by 100-fathom contours in the area outside of 100 fathoms depth, and 10-fathom contours within.

The interesting formation shown in figure 6 was surveyed in 1936 and lies just off the mouth of the Mississippi River.

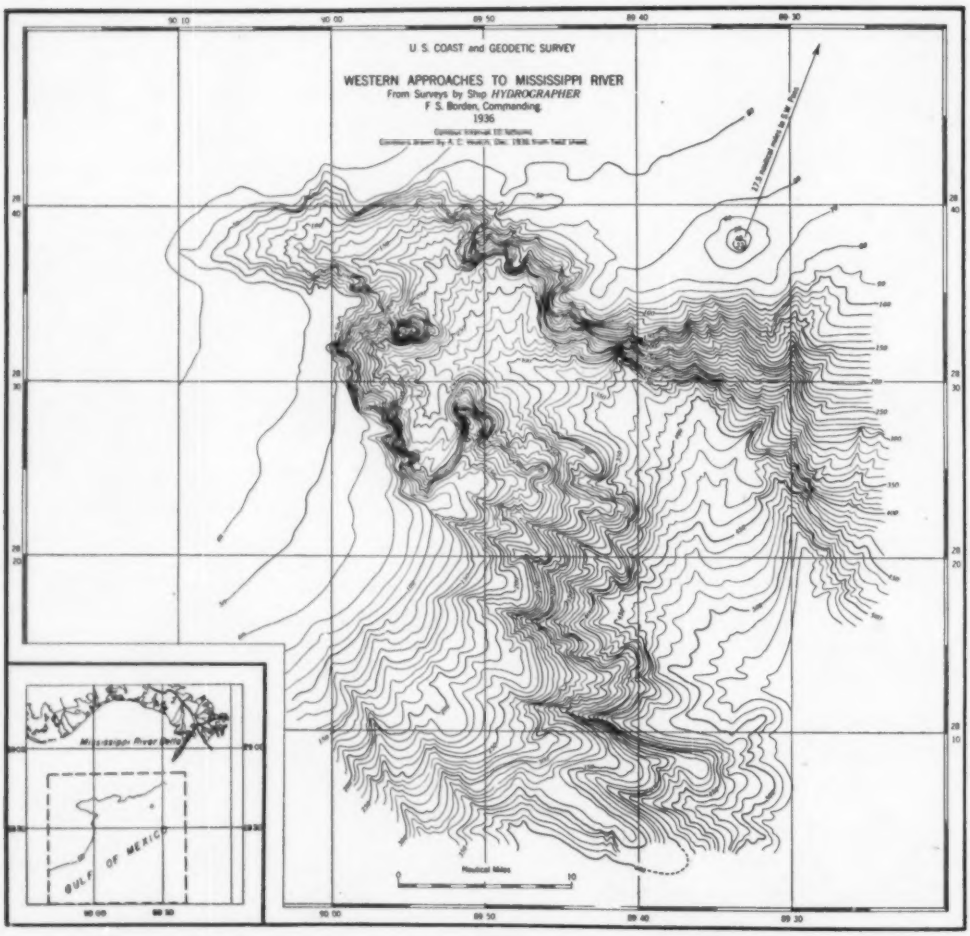

U. S. COAST and GEODETIC SURVEY
WESTERN APPROACHES TO MISSISSIPPI RIVER
From Surveys by Ship HYDROGRAPHER
F. S. Borden, Commanding.
1936
GULF OF MEXICO

FIG. 6. A submarine region off the Mississippi River Delta.

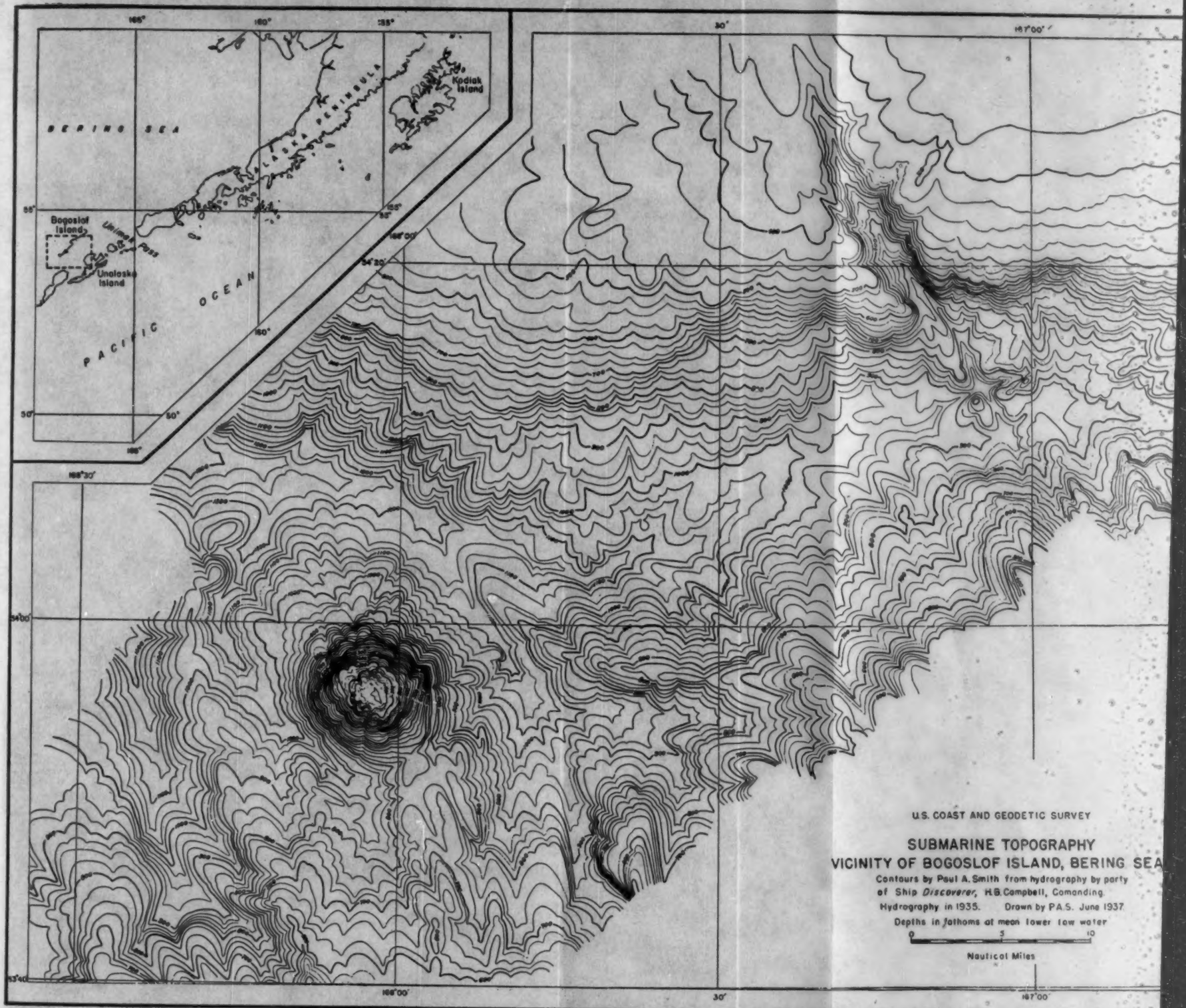
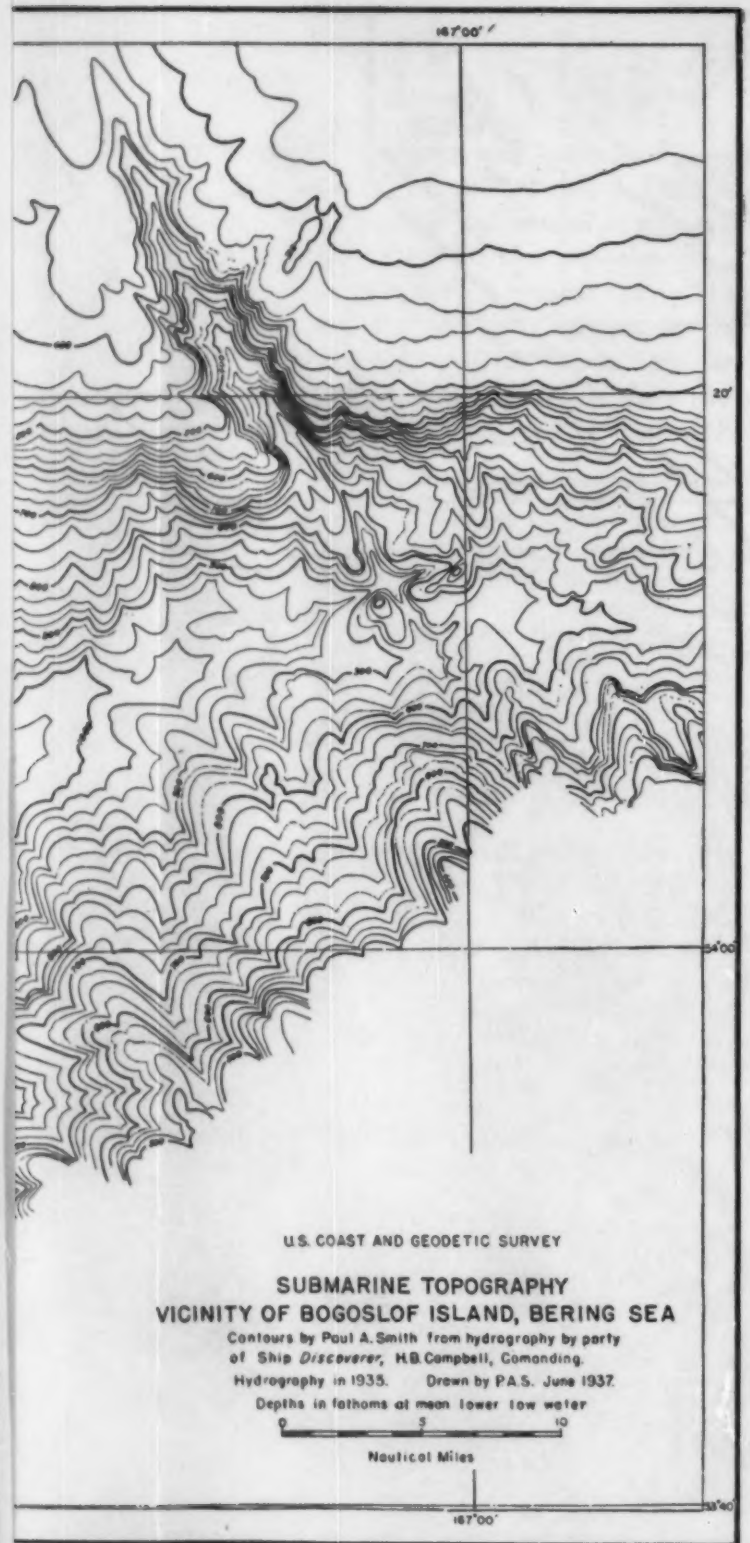

BERING SEA
ALASKA PENINSULA
Kodiak Island
Bogoslof Island
Unimak Pass
Unalaska Island
PACIFIC OCEAN
U.S. COAST AND GEODETIC SURVEY
SUBMARINE TOPOGRAPHY
VICINITY OF BOGOSLOF ISLAND, BERING SEA
Contours by Paul A. Smith from hydrography by party of Ship Discoverer, H.B. Campbell, Comanding.
Hydrography in 1935. Drawn by P.A.S. June 1937
Depths in fathoms at mean lower low water
Nautical Miles

FIG. 7. Submarine topography, vicinity of Bogoslof Island, Bering Sea.



land, Bering Sea.

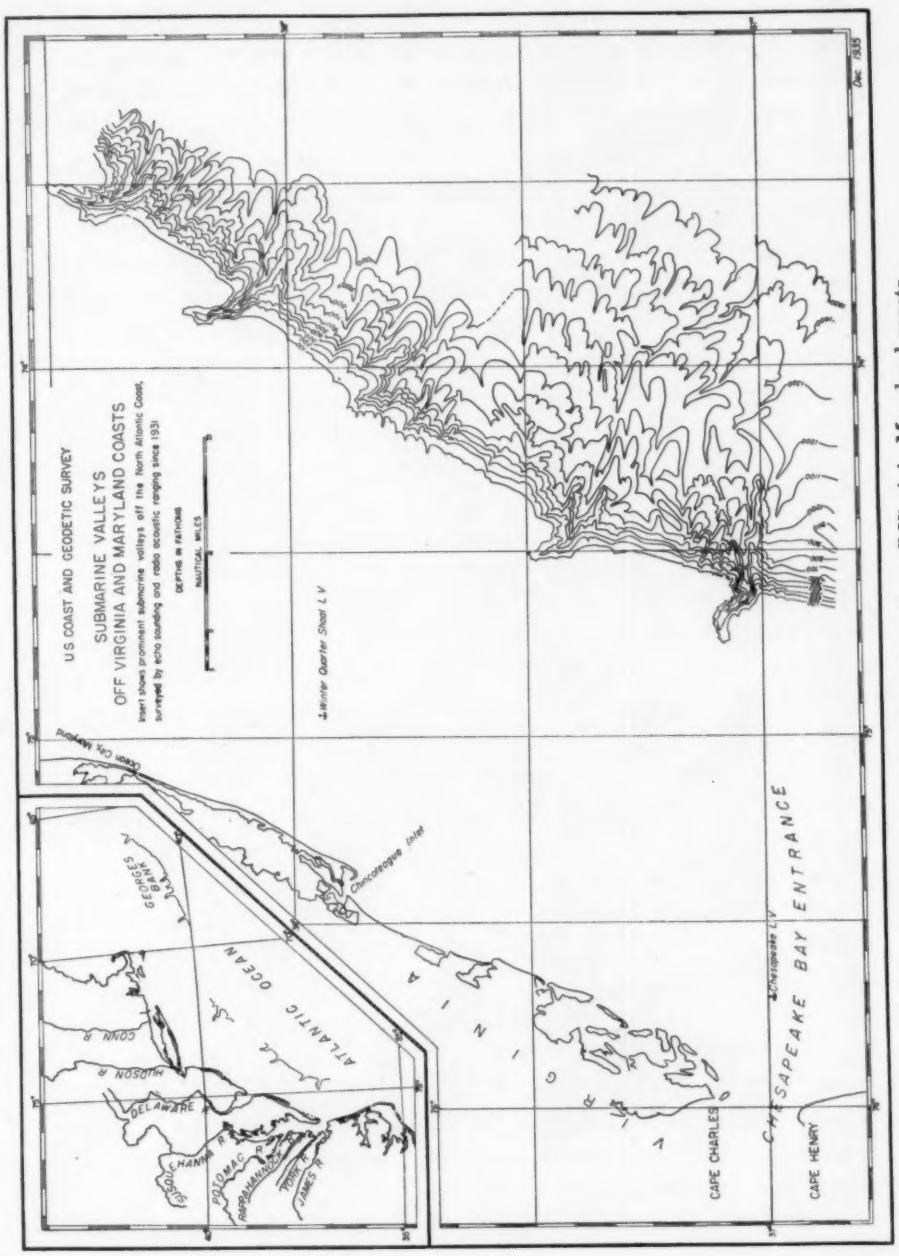


FIG. 8. General configuration off Virginia-Maryland coasts.

The small hill in latitude 28° 37′ N., longitude 89° 33′ W. has been interpreted as a salt dome uplift by several geologists. Numerous other similar formations have been found along the edge of the continental shelf in the Gulf of Mexico to the westward of this feature.

Figure 7 shows a region in the vicinity of Bogoslof Island, Aleutian Islands, Alaska. The contours on this plate are 25-fathom interval. It is of especial interest because it indicates a great volcano rising to the surface of the sea from the ocean floor over 5,000 feet below, around the base of which two large valleys are indicated. The valleys apparently were eroded by prehistoric streams subsequent to the formation of the volcano.

Figure 8 shows the general configuration of the Virginia-Maryland coasts.

The instruments developed in the course of these surveys have far-reaching significance in addition to their utilitarian value in coastal surveys. They supply a long felt need for equipment which will provide the facilities to future oceanographic expeditions for developing and surveying remote regions of the ocean. I am thinking especially of the sono-radio buoy, conceived and developed in the Survey. This little device can be built at a comparatively low cost, and expeditions equipped with these units can develop submarine features in oceanic areas in detail, using the velocities of sound in sea water as basic lengths. The velocities can be obtained by physical measurements of temperatures and densities. In conjunction with the fathometer, or other forms of echo sounders, a well equipped oceanographic expedition should have adequate means at its disposal to survey in detail any feature which may be of interest. The portability of the sono-radio buoy is illustrated in figure 9 showing a complete all metal unit used on the Pacific coast. The complete audio and radio parts with necessary batteries are contained in the small drum.

I can not commend too highly the valuable reconnaissance work in the deep ocean areas conducted by the United States

FIG. 9. All metal sono-radio buoy.

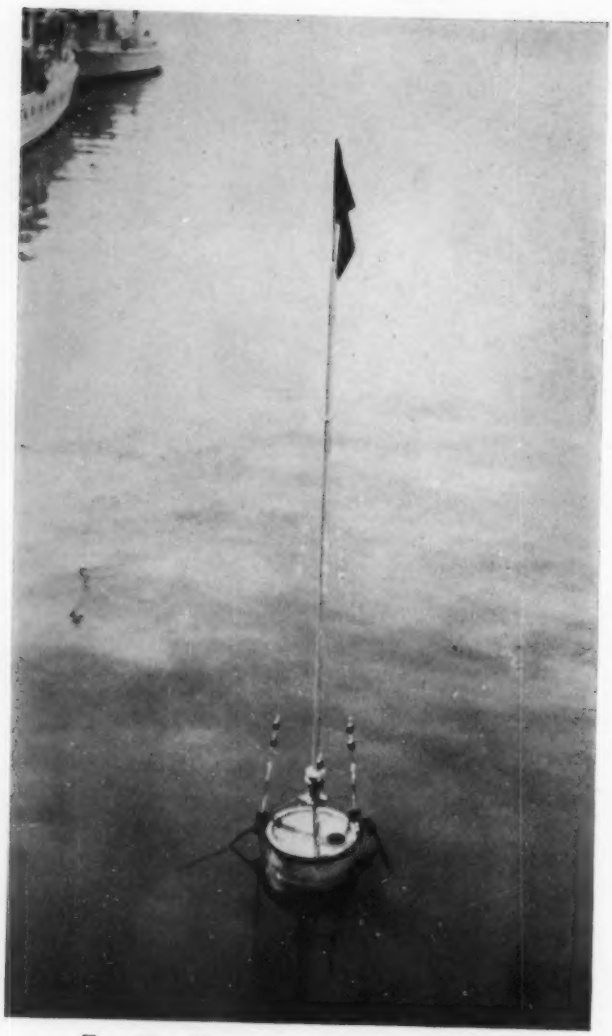


FIG. 10. All metal sono-radio buoy afloat.

Navy, particularly in the Pacific Ocean. This work has demonstrated the importance and the practicability of stimulating and encouraging merchant and naval vessels of other nations to record deep sea soundings on their passages across the oceans. The U. S. Coast Guard also has made valuable contributions to the preliminary knowledge of the submarine topography, especially in the Aleutian Islands region and the

FIG. 11. Complete audio and radio units of sono-radio buoy.

Bering Sea. In such a manner, submerged features of great significance lying outside the continental platforms can be discovered and later surveyed in detail by expeditions equipped with modern devices. Truly, we are in a new era of oceanographic exploration. The vast regions of the earth's surface lying beneath the sea which have so long been the subject of controversies and speculation soon must give up part of their secrets.

PRESENT STATUS OF THE PROBLEM OF SUBMARINE CANYONS *

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ABOUT three quarters of a century ago J. D. Dana (1), by commenting on the existence of a trough lying off the mouth of the Hudson River, may be said to have put before the geological world a problem to which we still do not possess the answer. His data were the relatively rough charts, to judge by their present day successors, furnished by the U. S. Coast and Geodetic Survey at that period. As more of these curious gashes were discovered in various parts of the world, geological discussion was stimulated, although on the older charts the form of the valleys was necessarily shown only as vague indentations in the continental slope (2, 3, 4). Here the matter rested for some years.

The recharting of Georges Bank by echo sounding was the spark which revived the question. For the first time a really adequate picture of the configuration of the valleys was available. It is hard to overestimate our debt to the U. S. Coast and Geodetic Survey for the continued development of this new technique, and for its application to offshore, deep-water surveying. Today we know the shape of these valleys with considerable exactness, we know their distribution, in some cases we know their age, but when the word origin is mentioned geologists can be counted on to line up in two opposing camps. It is not so much their size and world wide distribution which is disturbing, it is the as yet unanswerable problems to which they give rise. In order to account for their origin we are faced with the necessity of altering the relationship of land and sea to a seemingly

* Contribution No. 169 of the Woods Hole Oceanographic Institution.

impossible extent, or else appealing to submarine currents. It is becoming apparent, from the data which modern physical oceanography is accumulating, that the forces governing currents in the present-day oceans are not those which could produce the special types required for this work. A bottom current of a radically different type, brought into existence by the peculiar conditions of the Pleistocene, has been postulated by Daly. This will be discussed later. Some of the canyons have been dated by means of fragments of fossiliferous rock dredged from their walls, and they are found to be relatively young. Obviously erosional processes on a grand scale were in operation only yesterday, but the clues are just as obscure as though the cutting had taken place in the Cambrian.

The multiplicity of the theories and the extraordinary variety of processes, which have been called upon to produce the desired result, give ample testimony that the problem of canyon origin possesses no ready solution. The proposals range all the way from the interference of celestial bodies to density currents of an exceedingly complicated type. Let us now review the chief explanations which have been offered in the light of what little factual evidence we possess.

Before the charts of the recent surveys had been published, many considered the valleys to be tectonic rather than erosional features (5, 6). It is conceivable that a few of them can be explained in this way, but in every instance where an adequate survey has been carried out such has proved not to be the case. Faulting and folding never produce valleys of this shape, and they are certainly not the lunate scars of landslides. Most people will agree that they could only have been formed by running water, but where, under the air or under the sea? (7, 8.)

The advocates of subaërial origin have two alternatives. The land can be elevated or sea level can fall. Now that the maximum age of the canyons is known, the first hypothesis has developed so many obvious difficulties that it has few adherents today. The extensive grading and general adjustment to present sea-level which the continental shelves

display the world over, indicate that they are features of considerable antiquity. The canyons which notch their margins are so similar in general configuration and depth that contemporaneous formation is suggested. Consequently, under this hypothesis, it is necessary to infer that since the late Tertiary, the continental margins have been simultaneously elevated a matter of 10,000 feet or more, that they stayed there a short time, and then sank again to approximately the original level. The affect of such an enormous uplift on pre-existing drainage would have to be obliterated in the same short interval, since no record of any such dislocation remains.

Lowering sea level also has its drawbacks. It has been suggested that the bottom of one or more of the ocean basins sank and then returned to its former position, much like a piston in a cylinder. At present this point is safely beyond the reach of argument, for no one knows anything about the bottom of any one of the major ocean basins.

Hess and MacClintock (9) would change the shape of the hydrosphere, possibly by retarding the rotational speed of the earth, although they admit the difficulties of finding a satisfactory cause. Could this be effected, sea level would of course fall, below a certain critical latitude, with a consequent rise above it. They start with the premise that no canyons are found polewards of latitudes 55° to 60°. It has been argued however that 35° and not 55°–66° is really the nodal point (10, 11). If this is so, the hypothesis breaks down, for plenty of canyons are known north of 35°, and for that matter south as well.

One more method of lowering sea level has been suggested —the Pleistocene ice sheets. The commonly accepted estimate of the reduction of sea level by this means is of the order of 300 feet. Shepard (12), however, by enlarging known areas of ice accumulation, assuming an average thickness of four miles for the whole sheet, and chiefly by postulating an ice cap for the polar basin, raises the estimate to what the conservative considers the almost astronomical

figure of 3,000 feet. We have no yardstick to serve as a basis for discussion of much that he proposes. However, there are several factors in connection with the formation of an Arctic ice cap which deserve mention. This cap, to have any effect on sea-level, would have to extend to the bottom, a distance of two miles or more. Otherwise accumulating snow and ice on the surface would only sink the sheet deeper in the ocean, thereby displacing more water. Furthermore, the Arctic is today a region of little precipitation, and with a perpetually frozen ocean, surrounded by land masses covered with snow and ice, would it not be even more arid? At best accumulation would be thin compared to that farther south. In support of his hypothesis Shepard cites some recent work by the Russians showing that an ice sheet flowed southwards over northern Eurasia. However, the whole coast is bounded by an exceptionally wide continental shelf. The sea over this shelf is shallow, and, made even shallower during the Pleistocene by the withdrawal of water elsewhere, probably froze solidly to the bottom. Might not this platform be the center of accumulation for the southward flowing ice over Siberia?

Submarine currents of different types have frequently been suggested as the excavators of the canyons (13, 14, 15). In 1936, bottom currents in two of the Georges Bank canyons were measured by means of an Ekman meter (16). The normal, rotary system of tidal currents on the continental shelf is upset, and on the bottom the currents were found to flow up and down the valleys. Yet even here, where the currents at the surface are exceptionally strong, the highest recorded velocity was 11.08 cms. sec., or a little more than .2 of a knot at the peak of the flood tide. Spring tides at the surface have a velocity 40 per cent greater than the neaps, during the course of which the reading was made. If we arbitrarily increase this velocity by the above amount we get only 15.51 cms. sec. Considering the indurated nature of some of the beds of which this part of the continental shelf is composed, it does not seem probable that currents with

velocities such as these would be particularly competent agents of erosion. They would move the finer grade sizes in an unconsolidated deposit, and by so doing might serve to check or retard present day deposition, but this is the most effective rôle which they can be expected to play. The tidal currents were strong enough to completely mask other types which may have developed an on and off-shore component, and it is to be expected, in coastal waters, that they will always have the greater velocities. We are justified therefore, in eliminating tidal currents from the list of factors responsible for the east-coast canyons, and it is probable that further work will show this to be true for all canyons.

As was stated above, modern physical oceanographers tend to look with disfavor on the theory that these canyons could have been cut by present-day currents or by currents in the past actuated by similar forces (17). Localized currents with a strong on and off-shore component would be required, and aside from tidal currents, it has not been demonstrated that any such exist. Furthermore the conditions necessary for their production and maintenance are not fulfilled by the data which are gradually accumulating.

Daly has argued for a radically different type of density current (18). During the times of lowered sea level of the Pleistocene, large areas of unconsolidated, fine-grained deposits on the continental shelf and slope, which formerly had lain below wave base, came within the reach of wave action. Large quantities of sediment were thrown into suspension by the storms of that period. This heavier, denser water would first seek the depressions of the shelf. It would concentrate in them and run down the slope with greatly accelerated motion. Sufficient velocity might be developed to cut grooves, and later, trenches. Daly also considers the possible erosive effect of submarine mud flows which might be set in motion by the drag of this current of muddy water on the bottom. In a recent paper, Kuenen strongly supports the idea (19). He finds, by tank experiments, that currents of this type will cut grooves in an unconsolidated "slope," and

also that they do not diffuse readily with the surrounding water. There is no question but that this mechanism could operate in the open sea. The crux of the matter is, of course, the amount of energy available for the work translated in terms of velocities, and the hardness of the rocks which must be cut. The rocks of the Georges Bank canyons are known in more detail than those of any of the others (20). The upper part of the walls stand as cliffs with many outcropping ledges. The stratigraphic succession, as far as it is known, runs from the upper Cretaceous to the late Pliocene. A large proportion of these beds consist of clays, silts, and greensands which show little consolidation, but there are several beds of hard sandstone. Granite, as well as semi-consolidated sediments, has been reported from the walls of some of the California canyons (21). The presence of resistant rocks is the chief stumbling block which this theory has to overcome, and may prove a serious difficulty should many hard formations be found in future dredging.

The foregoing comments are not intended to be offered as purely destructive criticism, but to emphasize the point of view that there is no one universal explanation for the problem of canyon origin. The extreme differences of opinion which exist today are indications that in the future we will come to recognize a combination of causes. Also, it may well be that different canyons have been formed by different means. Should we assume a similar mode of origin for gorges cutting the seaward margin of broad, well-graded shelves; for those which head almost at the shoreline, and for those which lie at the outer end of a trench leading from some large river? Continued dredging and continued charting will help provide the answer. Without additional facts further discussion can not advance beyond the stage of hypothesis.

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THE TECHNIQUE OF SECURING UNDISTURBED CORE-SAMPLES OF THE OCEAN BOTTOM

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THE problem of securing core-samples of the bottom at depths as great as 1000 fathoms (6000 feet) is not particularly difficult. The mechanical and manipulative factors involved in making soundings down to this relatively moderate depth are not so great as to preclude the use of the kinetic energy of a heavy weight, or the employment of the heavy lines necessary to handle it; nor is the time element particularly significant. Beyond this point the difficulties multiply as the depth increases, until any attempt to bring up adequate cores from below 3000 fathoms (18,000 feet) is a feat of coördinated mechanical engineering and seamanship of no mean order. Kinetic energy can no longer be relied upon to do useful work at the bottom, and the various stresses and masses involved inevitably increase so much that efforts to keep them at a minimum assume a dominant importance.

The sounding cable itself develops complications of its own as its length increases. Hemp rope will grip a capstan-head easily and can be readily stowed in a relaxed condition beyond the hoisting mechanism, but the weight of any adequate apparatus, together with the strain of pull-out from the bottom, demands a rope whose bulk and buoyancy soon defeat its purpose. The cable ships meet this situation by covering steel wires with hemp, and using a capstan-head 7 feet in diameter, beyond which the rope is coiled in large tanks. In 1936 I used part of such a rope that was 20 miles long. Wire rope cannot be taken over a capstan-head under strain and then coiled satisfactorily. From the standpoint of strength and storage, wire rope is the most satisfactory, but

its weight brings up other problems. No wire rope is able to sustain its own weight of 6000 fathoms, required to sound the greatest deep (5900 fathoms), and retain any reasonable factor of safety, to say nothing of the weight of apparatus or the strains of the pull-out, or the movement of the ship at the surface. These considerations necessitate a tapered rope whose weight increases only in proportion to the strength required. This greatly increases the difficulty of spooling or winding on the winch drum, for unless spooling is smooth and regular in the underlying layers, overlapping ropes will be crushed by the enormous pressures developed. These same pressures develop great lateral stresses which tend to burst the flanges of the winch drum. Finally, winches of such great capacity are not common, and none exist on large, steady ships.

Therefore the problem of securing satisfactory core-samples of the bottom of the great deeps resolves itself into many mechanical considerations, involving the devising of a tool possessing a great supply of energy which is released and controlled entirely automatically on reaching the bottom, and which is lowered by means of a special cable and winch. The cores obtained must be undistorted, and the material must reach the laboratory in the exact condition in which it left the bottom. Furthermore, considerations of expense demand that this gear be adapted to use from any ship—not only from one especially built for the purpose. The coring apparatus itself must be as light as possible and yet very rugged. As it is not feasible to send any considerable energy down through much sea-water or to operate complicated machinery in this very corrosive substance, the apparatus must possess within itself the energy necessary to do the required work, and this energy must be reliably controlled and accurately directed. As the time element becomes increasingly important with increasing depth, this work must be accomplished as expeditiously as possible.

These essential features have been more or less completely met by an apparatus which has been in use since the

summer of 1935, and a special cable of 5400 fathoms and a winch to handle it are now being constructed. This coring apparatus consists of 5 principal parts: a gun, a cartridge, a firing mechanism, a water-exit port, and a bit. The mechanical details are fully described in the *Bulletin of the Geological Society of America*, Vol. 47, pp. 675–684, May 1936.

Sounding Procedure

The apparatus is assembled and aligned in chocks on the deck, the firing mechanism cocked, and the safety-pin inserted. A cartridge is loaded in accord with the anticipated need. If it is the first sounding in a new locality, it is advisable to provide rather less than the anticipated required energy. Subsequent loads may be increased as circumstances warrant. When all is ready, the cartridge is fastened in place and the gun "loaded," by sliding the bit toward the gun, and the shear pin is put in place. The apparatus is then hoisted over the side. A man picks up the bit at the cutting edge, and, as soon as the gun clears the rail, he drops his end over the side into the water. The apparatus is immediately lowered until only the gun is out of the water; this prevents swinging against the ship's side. Finally, the safety-pin is pulled out and the apparatus is lowered to the bottom. When the apparatus returns to the surface, if it has fired, the gun and bit will be hanging separately at the end of their respective cables, the bit supported in the stirrup. If they are not so separated, and it is desired to bring the apparatus aboard, the safety-pin must be inserted at the earliest possible moment while the apparatus is hanging clear of the ship. Once this pin is in place the gear may be brought aboard with safety.

The 14 cores shown in Fig. 1 were taken from the canyons off the Maryland-Delaware coast in 1935, and the 11 cores in Fig. 2 were obtained from the North Atlantic between Newfoundland and Ireland in 1936. These will be discussed by Dr. Bradley.

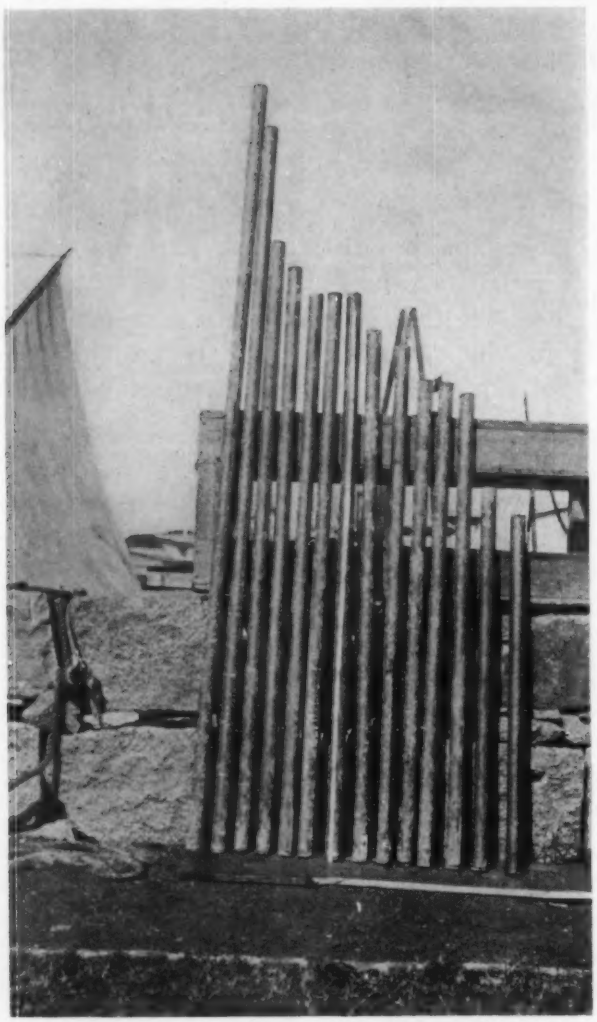

FIG. 1. Cores from the canyons in the continental shelf off New Jersey, Delaware, and Maryland. Maximum length 8 ft. 8 in. Depths 200–1250 fathoms.

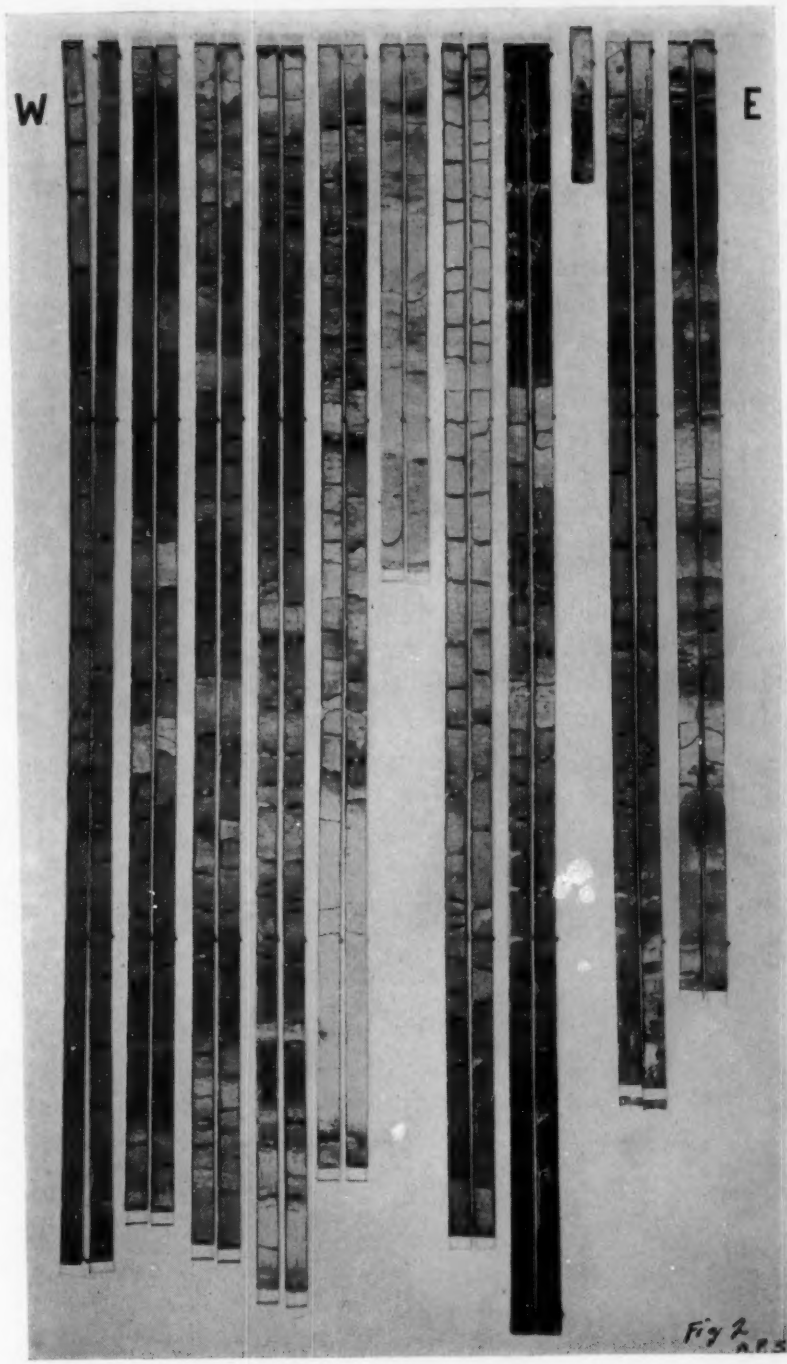

FIG. 2. Cores from the North Atlantic, Newfoundland to Ireland, arranged as taken from West to East. Maximum length 10 ft. Depths 700–2650 fathoms.

Cores have since been obtained from the eastern continental slope of the United States, and beyond, and also from the Bartlett Deep between Jamaica and Cuba at a depth of 2800 fathoms, and some have been taken from the Gulf of Mexico.

An essential requirement for a deep sounding is that the ship remain stationary with respect to the bottom. It is true that drift may then take the wire off somewhat, but this is a lesser evil than extensive movement of the ship by wind or surface current.

The best way to be certain that the ship *is* remaining stationary is to put out one or more buoys, despite the depth, and after the buoy has taken its permanent set to keep the ship oriented with respect to it. This is a laborious procedure, to be sure, and can only be done easily by a ship having ample power, and preferably two propellers. If a particular area is of special interest, and several soundings within a small radius are desired, the establishment of such a reference is well worth the added trouble.

THE GEOLOGICAL SURVEY'S WORK ON THE PIGGOT NORTH ATLANTIC DEEP SEA CORES*

W. H. BRADLEY, M. N. BRAMLETTE, J. A. CUSHMAN, L. G. HENBEST, K. E. LOHMAN, AND P. D. TRASK

The United States Geological Survey

A SERIES of 11 cores from the North Atlantic sea bottom between the Newfoundland Banks and the banks off the Irish coast have been studied by a group of geologists of the Geological Survey. These cores were taken by Dr. C. S. Piggot of the Carnegie Institution's Geophysical Laboratory from the cable ship "Lord Kelvin" with the explosive type of core-sampler, which he designed (1). This preliminary report is based largely on the results obtained by M. N. Bramlette, who studied the lithology, and Jos. A. Cushman, who studied the foraminifera.

The geographic position, the depth of water at each station, and the length of core obtained are given in table I.

TABLE I

GEOGRAPHIC POSITION, DEPTH OF WATER, AND LENGTHS OF CORES TAKEN FROM NORTH ATLANTIC SEA BOTTOM

Station and core No.	Latitude—N.	Longitude—W.	Depth of water in fathoms	Length of cores in meters
3	46° 03′ 0″	43° 23′ 0″	2575	2.82
4	48° 29′ 0″	35° 54′ 30″	2165	2.72
5	48° 38′ 0″	36° 01′ 0″	2640	2.82
6	49° 03′ 30″	32° 44′ 30″	2260	2.93
7	49° 32′ 0″	29° 21′ 0″	1780	2.62
8	49° 36′ 0″	28° 54′ 0″	700	1.24
9	49° 40′ 0″	28° 29′ 0″	2050	2.77
10	49° 45′ 0″	23° 30′ 30″	2295	2.97
11	48° 38′ 0″	17° 09′ 0″	2640	0.34
12	49° 37′ 0″	13° 34′ 0″	1770	2.43
13	49° 38′ 0″	13° 28′ 0″	1070	2.22

* Published with the approval of the Director, Geological Survey.

Core No. 3 was taken just off the Newfoundland Banks in the blue mud zone, but all the others were taken in parts of the ocean where the bottom is blanketed with Globigerina ooze. Core No. 8 was taken on the mid-Atlantic ridge and core No. 11 struck volcanic rock.

In about 20 representative samples from each core the percentages of calcium carbonate, clay and silt, sand, and coarse sand were determined. Also the relative abundance of foraminifera, coccoliths, and diatoms was estimated in these samples. Material between these guide samples was examined microscopically, especially in certain critical zones.

Two zones in which acid volcanic ash (refractive index near 1.51) is common were found. The upper one was found in all the cores except No. 11, but the lower one has thus far been found only in the lower part of cores 4–7. Although in most of the cores the upper ash zone is restricted to the upper part, in core No. 3 it is represented by scattered shards, but the core, despite its length, apparently did not reach that part of the ash zone where the shards are most abundant. This upper ash zone serves to correlate the cores and the lower ash zone, where found west of the mid-Atlantic ridge, reinforces the correlation.

Besides the zones of volcanic ash four other zones of distinctive lithology were found. These zones are characterized by a relative abundance of sand and pebbles, by a smaller percentage of calcium carbonate, and by a sparsity of organisms—foraminifera and coccoliths. They also have a distinctive texture. The pebbles are sub-rounded to angular and include a wide variety of rock types—sandstone, gneiss, soft shale, and limestone—of which limestone is the most common. Some of the pebbles are as much as 2 centimeters across. These zones are interpreted as glacial marine deposits formed during the Pleistocene glacial epoch when continental glaciers were eroding the land. Drift ice from the continental glaciers apparently transported considerable quantities of rock debris far out into the ocean basin. Larger boulders, some with

facets and striæ, have been dredged in latitudes as far south as the Azores, and have been interpreted as transported by Pleistocene ice drift.

Glacial marine deposits closely similar to these were described by Phillippi (2) from the South Atlantic off the margin of the Antarctic shelf ice. From that locality where they are now forming, Phillippi showed that they extended northward into the South Atlantic beneath a cover of first diatom ooze and, farther north, of Globigerina ooze.

In the equatorial part of the Atlantic, Schott (3) interpreted an abrupt change from a surface layer of warmer water foraminiferal fauna to an underlying colder water fauna as the boundary between post-glacial deposits and deposits of the last glacial epoch. In some cores he found, below the deposits characterized by a colder water fauna, other deposits containing a warmer water fauna which he interpreted as inter-glacial.

Between the glacial marine zones found in the North Atlantic cores the sediments consist chiefly of Globigerina ooze or limy clay much like that which is forming today in the same area.

The uppermost glacial zone is represented in all the cores but Nos. 3 and 11 and lies just below the upper volcanic ash zone. Core No. 3, though nearly 3 meters long, did not reach the base of the upper ash zone and so of course did not reach the top of even the highest glacial zone. Core No. 11 is evidently too short to have hit either the ash or the glacial marine deposits. In cores 4–7 the glacial zones are relatively thin and are spaced at approximately equal intervals. Between the third and fourth glacial zones (in descending order) is the lower volcanic ash. Only the uppermost glacial zone has been identified east of the mid-Atlantic ridge. Other glacial marine deposits are recognizable but their correlation is less certain.

Cores 3, 8, and 11 are markedly different from the others. Core No. 3 is distinctive for its uniformity in texture and

composition. With the exception of three thin clay layers that are widely spaced, this core consists of about equal amounts of clay and silt-sized clastic material and clay and silt-sized calcium carbonate particles. Coccoliths are rather common throughout, but foraminifera are relatively rare.

In contrast core No. 8 is characterized by an abundance of coarse sand and pebbles, by only the coarser volcanic glass shards which are not common, by abundant foraminifera, and by very little matrix of clay and silt sizes. According to our interpretation currents move across this part of the mid-Atlantic ridge (Faraday Hills) with sufficient velocity to winnow out most of the fine-grained material and carry it into deeper basins such as that where core No. 9 was taken. Core No. 9 is characterized by a predominance of very fine grained material.

Core No. 11 penetrated only about 15 centimeters of Globigerina ooze before encountering a deeply altered volcanic rock which it penetrated about 23 centimeters. Preliminary examination suggests that this rock may represent a submarine flow of basaltic rock.

It is evident from the large differences in depth below the ocean floor to the top of the latest glacial marine zone that the rate of deposition differs widely from place to place. In cores 4, 5, and 7, for example, the top of the latest glacial zone is about 30 centimeters below the top, and if the last glacial epoch is taken as 20,000 years ago then the post-glacial deposition in these three cores was at the rate of approximately 20,000 years per foot. As core No. 3 did not reach the glacial zone, the rate of deposition there must have been something more than 10 times as rapid. On the mid-Atlantic ridge the rate was somewhat slower and in some areas east of the ridge the rate seems to have been several times as rapid as prevailed generally west of the ridge.

Iselin (4) in studying some bottom cores off the Irish coast concluded that the marked decrease in carbonate content below about 25 centimeters represented deposits of a

glacial epoch. He estimated the rate of accumulation in that locality to be about 1 centimeter per 1000 years or 30,000 years per foot.

Schott (5), after studying the cores obtained on the Meteor expedition in the equatorial Atlantic, estimated the average rate of accumulation by the depth to the top of the deposits formed during the last glacial epoch to be 25,000 years per foot. His estimates for various cores ranged from 14,000 to 57,000 years per foot.

The biologic evidence now available is Cushman's interpretation of the temperature conditions revealed by the pelagic foraminifera at successive levels in the cores. He found that at some levels the foraminifera indicated warmer water conditions than prevail in that part of the ocean today, whereas at other levels they indicated much colder water than today. At other levels they indicated conditions nearly like those of today. Where the physical evidence indicates glacial marine zones the foraminifera indicate a cold water environment. The interglacial zones, except the latest one, all show warm water foraminifera faunas. The fact that the uppermost or latest interglacial zone contains a cold water fauna may mean that the uppermost two glacial zones indicated by the physical evidence are really only phases of one glacial stage—the last or Wisconsin stage.

Below the lowest glacial marine zone encountered in the cores the foraminifera indicate in each core conditions as warm as today or warmer.

From the evidence thus far available, we conclude that these long cores have penetrated sediments that represent not only all Post-Pleistocene time, but some have penetrated three, or perhaps four, glacial stages and three, or perhaps four, interglacial stages of the Pleistocene epoch. None of the foraminifera in the bottoms of the cores suggest Pliocene species.

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MARINE GRAVIMETRIC METHODS AND SURVEYS

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I. Introduction

In 1923 Dr. F. A. Vening Meinesz made a voyage from Holland to Java on board the Netherlands Submarine K II, during which he made pendulum gravity observations at 32 stations. Twenty-six of these were made while the submarine was submerged in the open ocean, and they are the first gravity measurements sufficiently accurate for modern geodetic purposes which were ever made at sea.

The great need of gravity data in oceanic regions had been pointed out by Bowie (1), Hayford (2), Bauer (3), and others. Many workers had attempted to measure gravity on ships at sea, but it had seemed so futile to attempt to operate a gravity pendulum on the unsteady base provided by a ship that the efforts had been directed toward development of some substitute to replace the standard pendulum method. Siemens (4) on the ship *Faraday* in 1875 used a gravity barometer in which the weight of a column of mercury was balanced against the pressure of an enclosed body of gas, and also a device in which a steel spring supported a mercury column. His observations are invalidated by causes not clear even now, but his instruments incorporated many principles now used in the most modern gravimeters.*

Hecker (5), using a method which Mohn (6) had developed for obtaining gravity corrections to barometer readings at stations of the Meteorological Service of Norway, made three extensive voyages between 1901 and 1909 on which he estimated the force of gravity by comparing the atmospheric

* It is a shock now to realize that Siemens was measuring gravity to compute from it the depth of water.

pressure determined from the boiling point of water with that given by a mercurial barometer. Bauer (3) carefully reviewed the work of Hecker while considering inclusion of gravity measurements in the program of the *Carnegie*, and concluded that "the problem of obtaining sufficiently reliable ocean gravity results still awaits solution." He expressed strongly the hope that the great importance of getting gravity values would lead someone to discover a better method for ocean gravity work.

Briggs (7) made measurements of gravity from Sydney to San Francisco in 1914, and from New York via Panama to San Francisco in 1915. His apparatus contained a gas maintained at constant temperature and volume, and he measured the height of the mercury column needed to balance the pressure.

Duffield (8) made gravity measurements during voyages from England to Australia and back in 1914, and again in 1922–23. He used gravity barometers, photographic recording barometers, and gravity spring balances, but his results apparently lack the accuracy required for geodetic work.

II. Development of the Meinesz Apparatus

From the very beginning of gravity measurements the pendulum has been the standard instrument for gravity surveys on land. All earlier workers had considered as axiomatic the impossibility of operating gravity pendulums on ships, but Meinesz approached the problem of marine gravity measurements with years of experience in pendulum gravity surveys. In the years 1912 to 1921 the Gravity Survey of Holland experienced difficulty in finding fixed bases for the pendulum apparatus at many stations, and the consequent unsteadiness had to be taken into account. By swinging two pendulums in the same plane for each observation, it was found possible to eliminate the effects of horizontal accelerations in this plane (9). The success of this effort led to an investigation of the possibility of using the apparatus on board a ship.

The first tests at sea were made on a small steamer on the North Sea Canal near Amsterdam with a Stuckrath four-pendulum apparatus. They showed the method to be feasible provided the visual observation of the coincidences could be replaced by photographic recording, and further that the motion of the ship could be held to a small value. Professor van Iterson suggested that a submerged submarine would be free from the disturbances of wave action, and a very successful test was made on a submarine in the roadstead at Helder in May 1923. Plans were then made for the voyage to Java in the autumn of 1923, in cöperation with the Netherlands Navy. On this voyage, which marks the real beginning of accurate gravity measurements at sea, pendulum gravity measurements were made with an accuracy approaching that of standard land measurements.

As a direct result of this work, over 1,200 gravity stations at sea have now been completed, more than 800 of them by Meinesz himself. Following the lead of the Netherlands Navy, the navies of the United States, U. S. S. R., Italy, France, and Japan have given support, each having participated in one or more surveys.

The Stuckrath apparatus used on the cruise of the K II was replaced in 1925 by a new instrument especially designed for measurements at sea. This apparatus underwent successive modifications, primarily in details such as gimbal suspension, mechanism of the photographic recorder, which were thoroughly tested on the cruises of the Netherlands submarines K XI and K XIII in 1925 and 1926. The form of the apparatus which has been accepted as final was reached prior to the cruise of the U. S. submarine S-21 in 1928. The final form of the apparatus is described by Meinesz (10). It contains three half-second bronze pendulums, as nearly isochronous as possible, which all swing in the same plane. It contains two auxiliary pendulums provided with internal damping which are intended to indicate the deviations of the apparatus from the vertical—one in the swinging plane of the gravity pendulums and one at right angles to it. The

case of the pendulum apparatus is well insulated against heat transfer but is not airtight. Inside the case are a hygrometer, a mercurial thermometer inside a dummy pendulum, and a bimetallic strip thermometer. By means of a mechanism operated from outside the case, it is possible to raise the pendulums and clamp them in spring supports when they are not in use, to lower them into the swinging position, to give them any desired initial amplitudes of swing, and to start them swinging in the same or in opposite phase as desired.

The light box, which contains the arrangement for photographic recording of the pendulum oscillations, is mounted directly above the pendulum case and the entire unit is supported in gimbals. The light box contains the supply of photographic paper, a spring motor for moving the paper during an observation, a lamp to provide illumination for recording, and shutters or light valves actuated by break circuit chronometers for making momentary eclipses of the beam of light. These eclipses serve as the time reference marks from which the periods of the gravity pendulums are calculated.

The beam of light which enters the pendulum case from the light box is divided into five parts, three of which are reflected from mirrors attached to the gravity pendulums. Let θ_1 and θ_3 represent the angular displacements of the two outermost pendulums, and let θ_2 represent that of the middle one. The optical system provides for continuous recording of the differences $\theta_1 - \theta_2$ and $\theta_3 - \theta_2$; also θ_2, the inclination of the middle pendulum to the vertical as indicated by the damped pendulum which operates in the swinging plane. The two other light beams record the temperature indication of the bimetallic strip and the tilt perpendicular to the swinging plane as indicated by the second damped pendulum. A photograph of the Meinesz apparatus is shown in figure 1.

The angular differences $\theta_1 - \theta_2$ and $\theta_3 - \theta_2$ are regarded by Meinesz to represent the angles of elongation of two fictitious pendulums, and it can be shown that, to the first order of small quantities, these fictitious pendulums are independent of horizontal accelerations in the swinging plane.

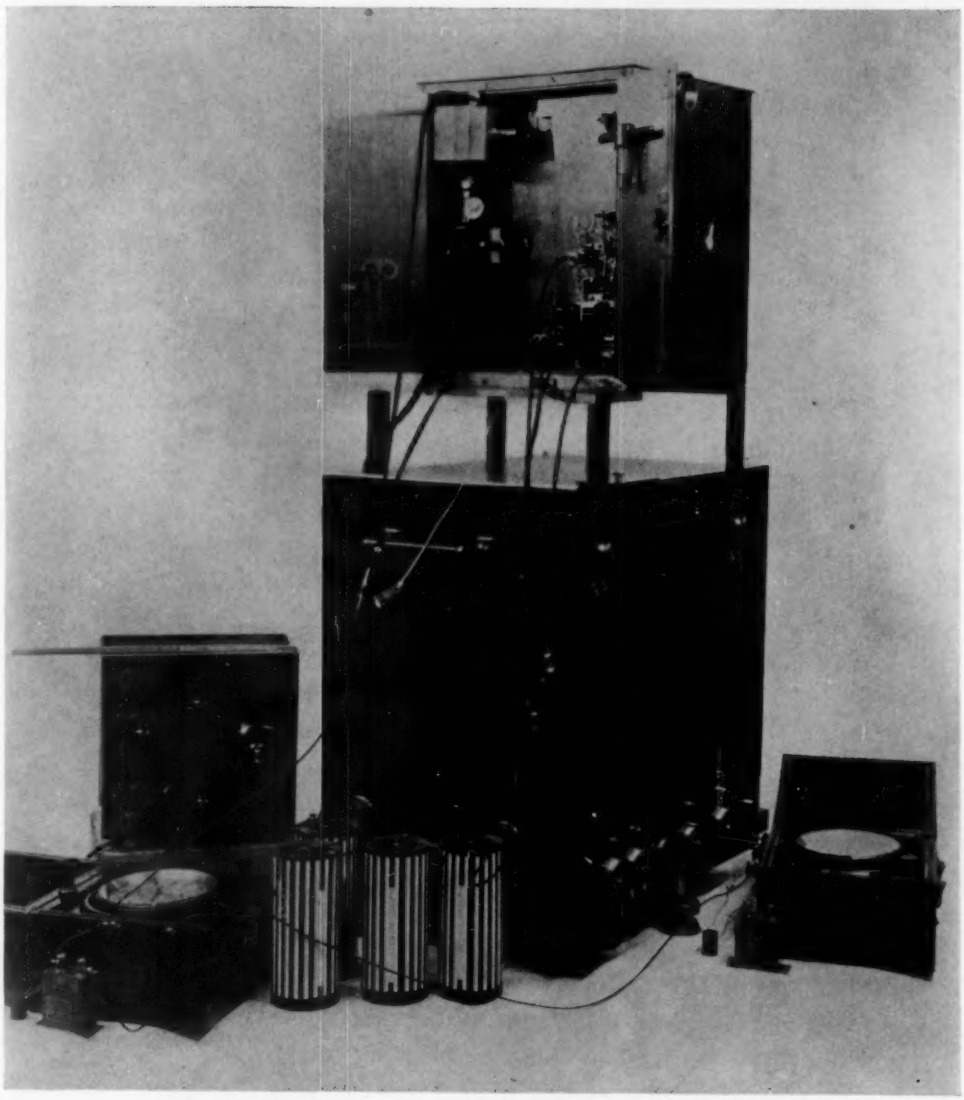

FIG. 1. Photograph of the Meinesz pendulum apparatus.

The chronometers used for measuring the periods of the pendulums produce eclipses in the light beams which record photographically the motions of the pendulums. The rates of these chronometers are determined by comparisons with radio time signals twice daily. Comparisons are made by connecting one of the break circuit chronometers in series with the headphones of the radio receiver and noting coincidences between the chronometer and the time signal. For rhythmic time signals, a mean-time chronometer was used, while for ordinary signals a mean time chronometer with rate deranged to give several coincidences during the period of time signal transmission was substituted. After reception of the time signal the chronometers were intercompared, also by a coincidence method.

The duration of a gravity observation with the Meinesz apparatus is about 30 minutes, and all observations are made with the ship submerged and travelling at constant speed and depth. It is necessary to know the magnitude and direction of the velocity of the ship in order to apply the Eotvos correction for the alteration in gravity produced by the east-west component of velocity. The depth of submergence depends upon the condition of the sea, and should be sufficient to reduce the effect of wave motion below a certain minimum value.

III. Summary of Surveys Using the Meinesz Method

The following table includes all marine gravity surveys known to the author on which the Meinesz apparatus, or instruments of other design embodying the principle of the fictitious pendulum, have been used. The complete results of all surveys have not yet been published. Information for each survey is listed under the following headings:

A. Date, name of ship, and number of stations occupied.
B. Personnel of scientific staff, and reference to publications.
C. Region surveyed.
D. Description of apparatus used.

1. 1923, Sept. 18–Dec. 24 Netherlands K II Number of Stations 32
Meinesz; Reference 11, Vol. 1, Chapter 2
Holland via Suez to Java
Apparatus: Stuckrath, 4 bronze pendulums swinging in 2 planes. Photographic recording which recorded motion of each pendulum separately. Cradle to eliminate effects of ship roll added in Gibraltar. One chronometer used with relay between it and the light valve.

2. 1925, Oct. 15–Nov. 12 Netherlands K XI Stations 12
Meinesz; Reference 11, Vol. 1, Chapter 3
Holland to Alexandria
Apparatus: Meinesz 3 pendulum design, recording fictitious pendulums directly, decreasing labor of reduction. Accuracy increased by use of 2 chronometers. Unnecessary to open pendulum case for any operation. Light box separate from pendulum case with cradle for removing roll of ship. Echo soundings adopted as standard part of program on this voyage.

3. 1926, May 27–Dec. 13 Netherlands K XIII Stations 128
Meinesz; Reference 11, Vol. 1, Chapter 4
Holland via Panama to Java
Apparatus: Meinesz, improved by complete gimbal suspension, light box atop pendulum case, cable release for shifting paper speeds, and agate bearings and steel points on damped pendulums to decrease friction.

4. 1927, Feb. 3–Feb. 15 Netherlands K XIII Stations 28
Meinesz; Reference 11, Vol. 1, Chapter 4
Java Deep
Apparatus: Same as for No. 3

5. 1928, Oct. 2–Nov. 27 United States S-21 Stations 49
Meinesz and Wright; Reference 12
Washington to Greater Antilles, Gulf of Mexico, and return
Apparatus: Meinesz, with improved light box. Light valves

operated on smaller current, eliminating necessity for relays between light valves and chronometers. This form of the Meinesz apparatus was accepted as final.

6. 1929, June 12–Aug. 12 Netherlands K XIII Stations 232
Oct. 8–Nov. 14
1930, Jan. 2–Feb. 15
Netherlands East Indies
Meinesz; Reference 11, Vol. 1, Chapter 5
Apparatus: Meinesz.

7. 1929, Sept. 3–Nov. 29 *Carnegie* Stations 5
Forbush; Reference 13
San Francisco to Apia
Apparatus: Meinesz, purchased in Holland. Apparatus used in surface vessel really successful only in harbors. Verifies conclusions of Meinesz about use on surface vessels after experiments in 1924, 1925, and 1929.

8. 1930, Aug. 2–Sept. 5 Russian Navy Stations 31
Sorokin; Reference 14
Black Sea
Apparatus: Fechner 4 pendulum, with photographic recording and modified optical system to give 2 fictitious pendulums as in the Meinesz apparatus. Invar pendulums used with the statement that magnetic fields were found to be less variable inside the submarine than on land.

9. 1931 Italy *Vettor Pisani* Stations 100
Cassinis and DePisa; Reference 15
Mediterranean
Apparatus: Meinesz, borrowed from Holland

10. 1931, Aug. 18–Sept. 8 *Nautilus* Expedition Stations 7
Villinger: Reference 16
Arctic Ocean
Apparatus: Meinesz, purchased from Holland.

11. 1932, Feb. 7–Mar. 19 United States S-48 Stations 54
Meinesz, Hess, and Brown; Reference 17
Bartlett Deep
Apparatus: Meinesz, with new pendulums constructed in 1931 on the minimum principle.

12. 1932, July 5–Aug. 14 Netherlands O-13 Stations 61
Meinesz; Reference 11, Vol. 2, Chapter 2
Mid-Atlantic Ridge near Azores
Apparatus: Meinesz, with the pendulums of cruise 11.

13. 1932, Oct. Japan Ro-58 Stations 5
Matuyama; Reference 18
Sagami Bay
Apparatus: Meinesz, purchased from Holland.

14. 1932, Nov. 10–1933, Jan. 10 France *Fresnel* Stations 30
Marti; Reference 19
Mediterranean, western part
Apparatus: Meinesz, borrowed from Holland.

15. 1933, Oct. 17–Nov. 8 Russian Navy Stations 38
Sorokin; Reference 14
Black Sea
Apparatus: same as cruise 8.

16. 1934, Nov. 14–1935, July 11 Netherlands K XVIII Stations 230
Meinesz; Reference 20
Holland to Brazil to Cape Town to Australia to Java
Apparatus: Meinesz

17. 1934, Oct. 17–Nov. 1 Japan Ro-57 Stations 25
Matuyama; Reference 18
Japan Deep, middle part
Apparatus: same as for cruise 13.

18. 1935, July and August Italy *Des Geneys* Stations 47
Cassinis; Reference 21

Mediterranean, Western part
Apparatus: Meinesz, purchased from Holland. Boat steered in the meridian during all measurements to avoid Eotvos correction.

19. 1935, Oct. 12–Oct. 29 Japan I-24 Stations 29
Matuyama; Reference 22
Japan Deep, southern part
Apparatus: Meinesz as in cruise 13.

20. 1936, Nov. 29–1937, Jan. 14 United States *Barracuda* Stations 57
Ewing, Hoskinson, and Hess; Reference 23
Panama through Lesser Antilles to Philadelphia
Apparatus: Meinesz apparatus used in cruise 10. Crystal controlled chronometer used, placing time reference marks directly on record without use of light valve. Half second interval between eclipses to avoid necessity for finding axis of recorded pendulum curve. Period and phase readable over entire record.

21. 1937, Jan. 11–Mar. 16 Netherlands O-16 Stations 93
Meinesz; Reference 24
Holland to Washington to Lisbon
Apparatus: Meinesz, three chronometers, two recording on the pendulum record and all three intercompared by a small recording apparatus before and after each gravity measurement.

IV. Modification of Meinesz Apparatus by Later Workers

Tsuboi (25) has described a new device for damping free vibrations of the pendulum case in the gimbal suspension.

On the cruise of the U. S. Submarine *Barracuda* in 1936 (23) an alteration of the method of timing the pendulums was introduced. A crystal-controlled oscillator supplying alternating current of constant frequency was used to drive a small synchronous motor, which served as a chronometer. This

chronometer, which was constructed by the Bell Telephone Laboratories under the supervision of Mr. W. A. Marrison, possessed a rate so constant that uncertainty in gravity results caused by fluctuations in chronometer rate were entirely negligible.

This motor was mounted in the light box of the Meinesz apparatus. It contained a gear, turning at 2 revolutions per second, which produced a momentary eclipse of the light beam on each revolution. Thus there were no light valves, which conceivably introduce slightly variable lags, interposed between the chronometer and the recorded time reference marks. The chronometers used in all previous work had likewise produced two eclipses per second, but the two parts into which each second was divided were not rigorously equal. Therefore one set of the signals had to be disregarded in determining coincidences between the chronometer and the fictitious pendulums, and coincidences were measured on the pendulum record by the intersection of the line of eclipse marks with the axis of the sine curve for the fictitious pendulum. The crystal chronometer gave eclipses rigorously one half second apart, and coincidences were measured by the intersection of the ascending and descending lines of eclipse marks, increasing the accuracy and decreasing the effort required for making the measurement.

One of the light valves of the standard Meinesz system was retained in the *Barracuda* measurements. It was connected to an ordinary chronometer which caused it to eclipse the light for one half second in each alternate second. These long eclipses were not used in measuring the periods because of the inferior constancy of rate of the ordinary chronometer. They were used to make visible the phase relationship between the fictitious pendulums and the middle pendulum, and as they occurred only in alternate seconds they did not interfere with measurements of the coincidences. For the entire duration of the observation the paper was moved through the light box at the high speed which had been used only at the beginning and at the end of the observing period in all

earlier work. Thus both the coincidences and the phase relationships could be measured for any instant during the entire observation. Several times daily the crystal chronometer was compared with radio time signals from station NAA by use of an apparatus * which produced a momentary flash of light at the beginning of each time signal. The gear of the synchronous motor mentioned above, which was graduated in intervals of 0.01 sec., was viewed in the light from the Strobotac. The comparison with the time signal could easily be carried out with a probable error of 0.002 sec.

V. Second Order Disturbance Terms

In a recent paper of great importance for marine gravity measurements, Browne (26) pointed out the necessity for making a correction, involving second order terms for disturbances caused by motions of the ship, which had been overlooked by all previous workers. The principal disturbances for which this correction is needed when the observations are made on a submerged submarine seem to be those caused by the very long waves or swells. These swells have periods several times longer than the periods of the gravity pendulums and their effect is readily apparent on the pendulum records, where it produces slow fluctuations in the line of eclipse marks. The procedure previously used corrects only for the first order effect of these accelerations.

Let $\ddot{x}$ represent the acceleration of the apparatus in the vertical direction, and $\ddot{y}$ and $\ddot{z}$ the horizontal accelerations perpendicular to the axis of the ship and parallel to it, respectively. Then the second order disturbance is given by

$$g = -\ddot{x}^2/4g + \ddot{y}^2/2g + \ddot{z}^2/2g,$$

where the dashes indicate values over the entire duration of the observation. Vertical accelerations cause the observed values of gravity to be too small and horizontal accelerations cause them to be too large. Consideration of the shape of a

* Manufactured by the General Radio Co., Cambridge, Mass., under the name Strobotac.

ship indicates at once that the accelerations $\ddot{z}$ parallel to the ships axis will probably be very small. The vertical accelerations may be read directly from the pendulum record as stated above. The horizontal accelerations perpendicular to the axis of the ship are probably of the same order of magnitude as the vertical accelerations when the course of the ship is perpendicular to the direction of travel of the swells, and is probably negligibly small when the course is parallel to this direction.

Meinesz (27) has already undertaken an investigation of the accelerations in a submerged submarine for the two-fold purpose of learning how best to apply the correction to the observations which have already been made, and for testing a device which he proposes to add to the pendulum apparatus to record the necessary data for making the correction in all future measurements. He gives a preliminary estimate of the importance of the correction for the older work, stating that it will amount to less than a milligal on very many stations, particularly for those located in enclosed seas where large swells are less common than in the open ocean. This result is confirmed by the records of the *Barracuda* cruise, where vertical accelerations were found to be almost imperceptible in the Caribbean, but definitely present for stations in the Atlantic. Meinesz finds that in extreme cases, such as were encountered by him in the Netherlands 0–16 while crossing the Atlantic in 1937 with exceptionally rough weather, the correction may reach 30 milligals for the vertical accelerations alone. The correction for horizontal accelerations $\ddot{y}$ will be opposite in sign and, under the opinion stated earlier in this section, will range in magnitude from zero to about twice the value of the correction for vertical accelerations depending on the relation of the course of the ship to the course of the swells. For almost all of the *Barracuda* stations the course of the ship was parallel to the direction of propagation of the swells, so it may be expected that the second order corrections will lead to values for gravity slightly greater than those given in the preliminary report (23).

VI. Negative Gravity Anomalies—Meinesz Belts

As is well known, the gravity anomaly at a station is the excess of the measured value of gravity over the theoretical value given by a standard formula. We may consider that the formula was selected to make the anomalies come out as small as possible. If the theoretical value of gravity has been corrected for topography and for isostatic compensation of the topographic features before comparison with the observed value, the anomaly is called an isostatic anomaly. Isostatic anomalies are interpreted to mean that there exists, near the station, an excess of mass over that assumed for isostatic compensation of the topographic features, while negative anomalies indicate a deficiency of mass.

A great narrow belt of large negative gravity anomalies, which might well be called a Meinesz belt in honor of its discoverer, was found in the East Indies and elaborately mapped by Meinesz in the years 1926–1929. It is a curved zone over 5000 miles long and usually not more than 60 miles wide, which winds through the entire East Indian island arc as shown in figure 2. It is almost continuous throughout its entire length, and on almost any section across it the negative gravity attains an extreme value of 100 to 200 milligals. The belt of negative anomalies shows a close relationship to the tectonic features of the region, coinciding almost exactly with the belt of earthquake epicenters where many earthquakes of the strongest type occur. The volcano belts lie on curves on the concave side of the belt of negative anomalies at a distance of about 100 miles. Without exception the typical deep sea troughs of the region are parallel to the Meinesz belt, lying near to it on its convex side.

A second Meinesz belt has been mapped in the West Indies, extending from Cuba around the island arc to Trinidad, as shown in figure 3. This belt was discovered by Meinesz in 1926, and was further studied during the cruises of the U. S. Submarines S-21, S-48, and *Barracuda*. It is similar in all major respects to the belt in the East Indies.

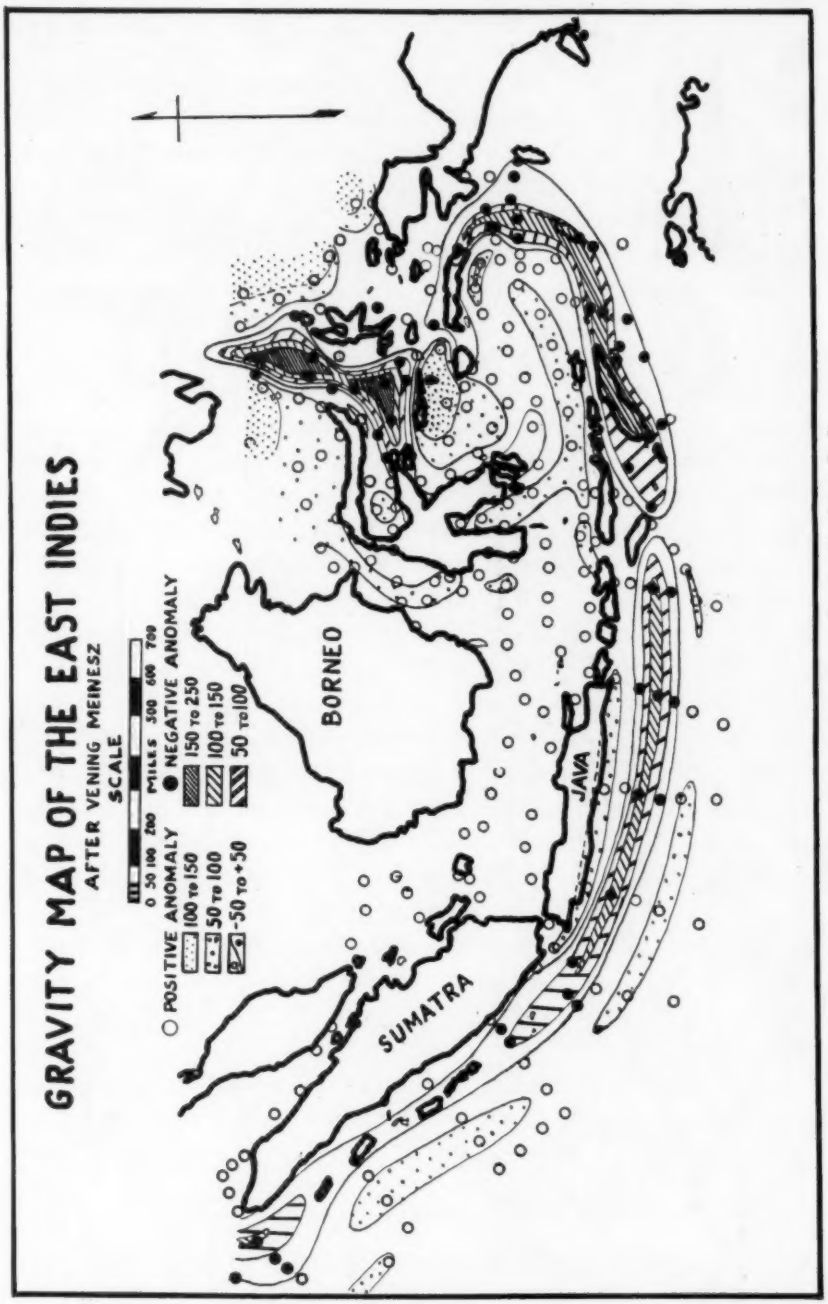

FIG. 2. The gravity anomalies in the East Indies.

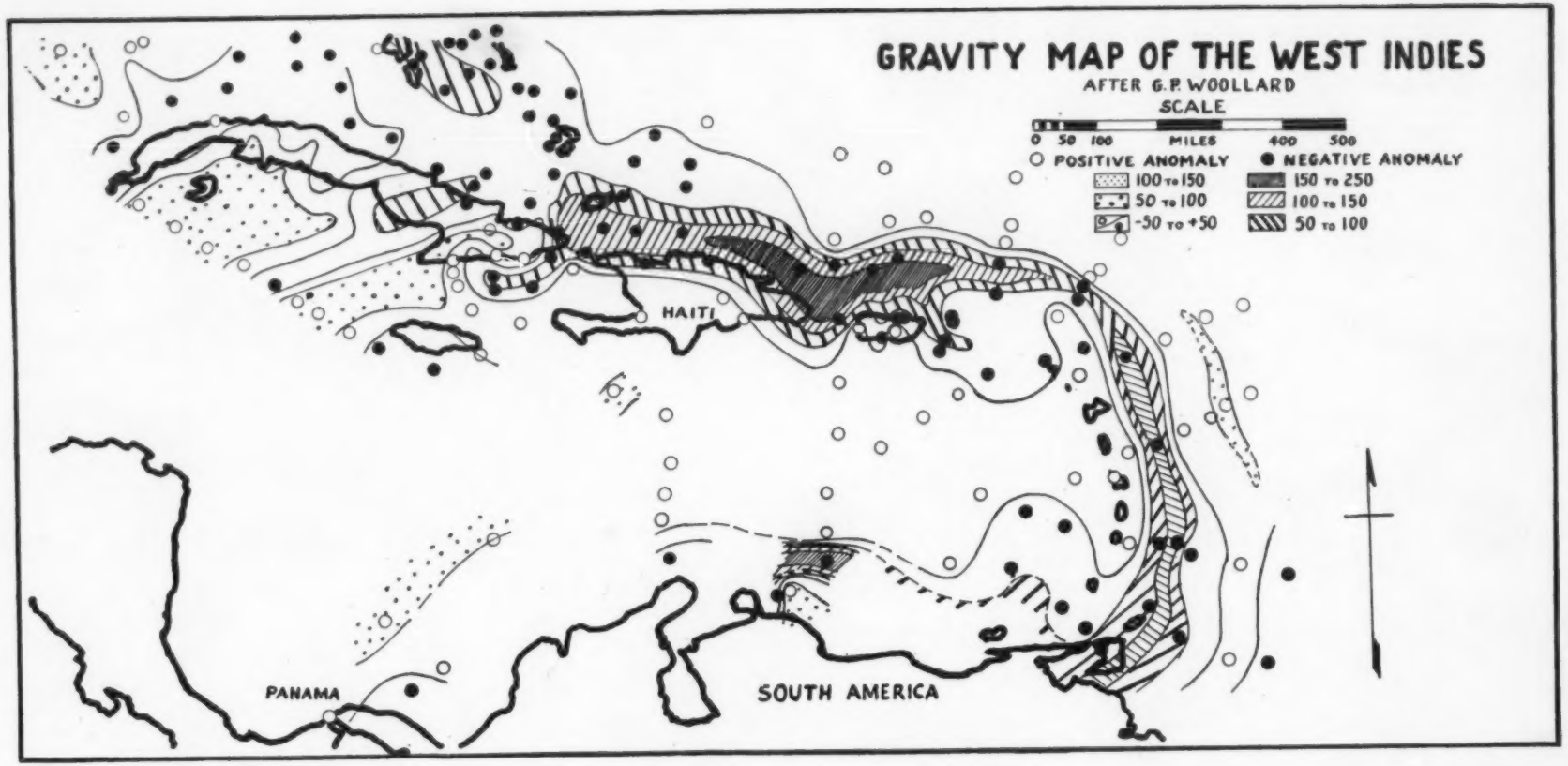

FIG. 3. The gravity anomalies in the West Indies.

A third belt has been found associated with the Nippon Trench as shown in figure 4. It has been surveyed by the Japanese expeditions between 1932 and 1935 for a length of about 900 miles. This belt also resembles the East Indian one in its major features.

These narrow belts of strong negative gravity anomalies are probably the most striking features of the gravit8tional field of the earth which have yet been discovered. The close relation which they show to tectonic phenomena indicates that they will give much information about the structure of the crust of the earth and about the processes of mountain building. The actual values obtained for the anomalies of gravity in these regions are largely independent of the system of isostatic reduction chosen for arriving at the isostatic anomalies, which proves that the results may not be attributed to an incorrect assumption about the distribution of compensating masses. The explanation advanced by Meinesz is the "buckling hypothesis," which postulates a downward protuberance of crustal material into the denser substratum. This is considered as the result of buckling of the crust under compressional stresses. The light crustal material is prevented from rising upward and attaining isostatic equilibrium by the continued action of the compressional stresses. Taking the density of the crust to be 0.6 gm./cm.^{3} less than that of the substratum and the crustal thickness to be 25 km., Meinesz concludes that a crustal shortening of 50 km. will suffice to produce the observed gravity anomalies in the East Indies. For a complete discussion of the question, the reader may consult the works of Meinesz and his colleagues (11, 28) and Hess (17, 29).

It now seems probable that every "island arc" has an associated Meinesz belt of negative gravity anomalies, and it is important that additional surveys of island arcs should be made.

VII. Positive Anomalies Over the Oceans

In general positive anomalies of gravity are found over the oceans. These are much smaller than the negative

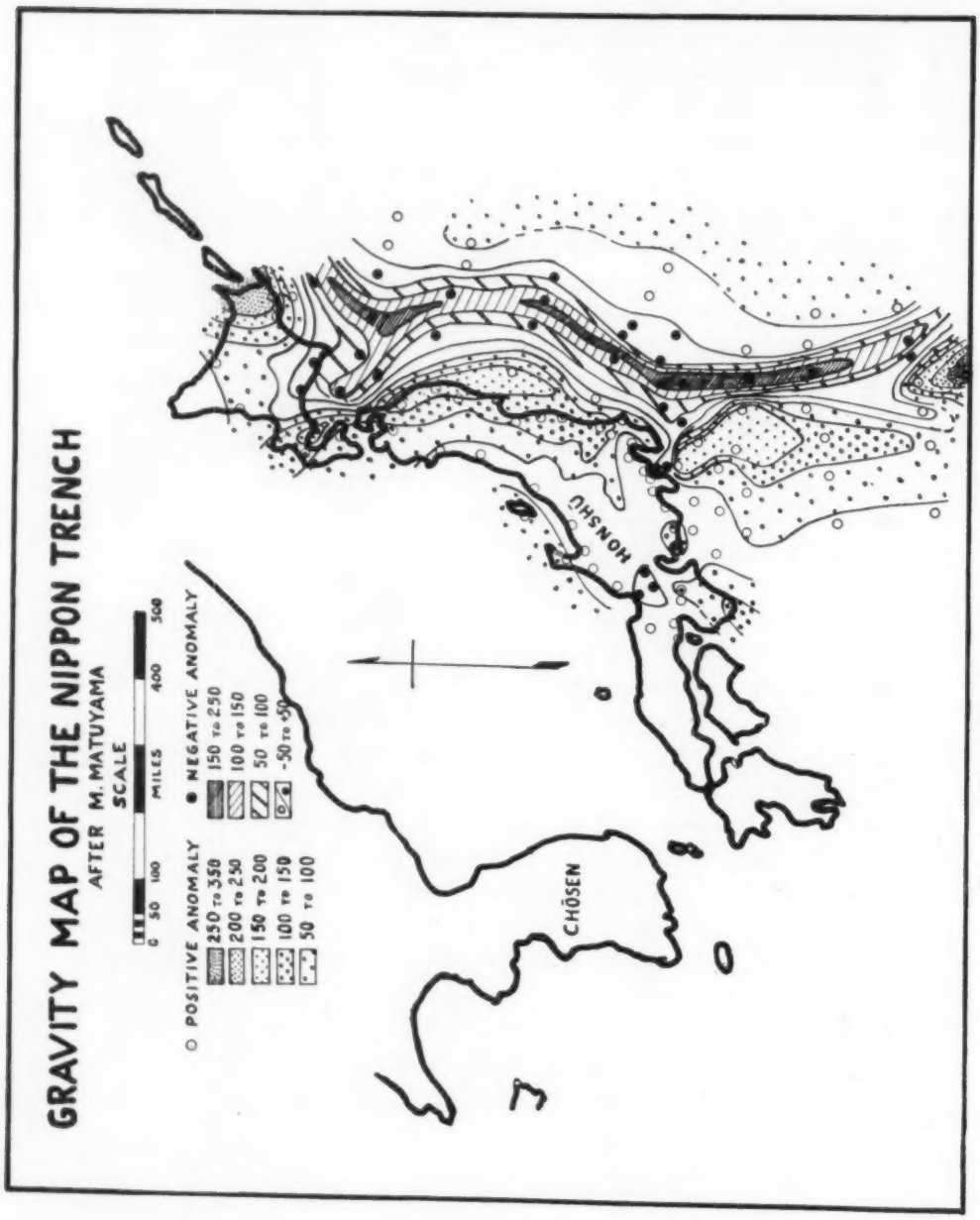

FIG. 4. The gravity anomalies over the Nippon Trench.

anomalies described in the last section, averaging about 22 milligals in the Pacific Ocean and about 41 milligals in the Atlantic. Meinesz has offered the explanation that convection currents in the substratum, upward under the continents and downward under the oceans, produce these large fields of positive anomalies. To explain them in terms of compressional forces in the crust would require such great stresses in the crust that this alternative explanation is rejected. Hales (30) and Pekeris (31) have published calculations showing the possibility of quantitative explanation of the oceanic anomalies by thermal convection currents in the substratum.

It is conceivable that the second order corrections introduced by Browne will remove these positive anomalies over the oceans, but it seems more probable from the discussion of Section V that the correction will eventually prove to be in the opposite direction. Dr. Bullard reports that preliminary measurements by Browne indicate approximate equality of horizontal and vertical accelerations in deep water.

VIII. Static Gravity Meters for Marine Measurements

As was mentioned in the introduction, all attempts to measure gravity at sea prior to the work of Meinesz were made with static gravity meters, or gravimeters, instead of pendulums. With the vast improvements which have been made in gravimeters, largely by those using them in prospecting for oil, it is natural that further attempts to adapt these instruments to marine measurements should be made. Norgaard (31) has described a static gravimeter, reading to 1 or 2 milligals on land, with which he made tests at sea on a surface ship in the Baltic Sea in 1932 and 1933. His instrument is in principle a modified Cartesian Diver floating in oil, and provided with temperature compensation. It did not work very well in a rough sea, but there is a good chance it would be useful for observing in a submerged submarine.

Haalck (32) made gravity measurements in 1934 on ships on the Elbe and on a voyage on a surface vessel from Hamburg

through the North Sea and the Baltic Sea to Finland and return. He was able to make measurements in the sheltered parts of the route but was not successful in the open sea. His instrument is constructed on the barometer principle, and has certain points of resemblance to one of the instruments built by Siemens. It was developed to make measurements on land with a probable error of 2 or 3 milligals. His successful measurements in protected bodies of water give him confidence that the instrument can be made to give satisfactory results on a surface ship on the open ocean. The recent work on second order effects discussed in Section V proves that measurements on surface ships will be even more difficult than was previously supposed, but it seems likely that Haalck's instrument would function properly in a submerged submarine.

An outstanding difficulty for all static gravimeters for marine measurements is that they do not as a rule enjoy freedom from shifting of the zero-point to the extent that pendulum instruments do. Therefore static gravimeters need to be returned to a base station at frequent intervals, which is a much more serious problem for marine than for land work. It was originally planned to use a static gravimeter in conjunction with the Meinesz apparatus on the cruise of the *Barracuda*, but this plan could not be carried through.

IX. Gravity Measurements in Polar Regions

While the *Fram* was frozen in the ice and drifting in the Arctic Ocean from 1893 to 1896, pendulum observations were made in the ship and also on the ice near the ship by Scott-Hansen. The results have been published by Schiøtz (33) for seven stations. A von Sterneck two-pendulum apparatus equipped with a coincidence apparatus was used, but apparently the results are inaccurate due to the effect of tremors in the base on which the apparatus rested.

In the voyage of the *Maud*, 1918–1925, Sverdrup (34) made an observation near Cape Chelvuskin with the same

apparatus used on the earlier expedition. He gives the mean error as 6 milligals.

A highly important contribution to marine gravity surveys is now being made by the floating expedition of the U. S. S. R. under the leadership of I. D. Papinin. Four men were landed on the ice at the North Pole by means of an airplane on May 21, 1937, and a camp was established for them. On February 5, 1938, the camp had drifted more than 1100 miles, and was near the coast of Greenland at north latitude 74° and west longitude 16°. The party was in danger because the ice floe was breaking up, and rescue parties were attempting to reach them. As part of an extensive scientific program, 13 gravity expeditions had been made at the time of the latest report (35). It is probable that these observations were made with an apparatus embodying the principle of the Meinesz fictitious pendulum, as described by Sorokin (14) in the report of his excellent work in the Black Sea, in which case the gravity results will not be prejudiced by tremors in the ice. This work is valuable for it promises gravity data in a region where observations are badly needed, and it introduces a feasible method which can eventually give gravity surveys over the entire extent of the polar seas.

The possibility of using modern airplanes to move an expedition from station to station on a polar ice sheet has placed a comprehensive survey of the polar seas within our reach, just as the development of submarines capable of making long ocean voyages made possible gravity surveys of the other parts of the oceans.

X. Relation to Isostasy and the Figure of the Earth

The results of the marine gravity measurements have provided a direct confirmation of the principle of isostasy. The mass-defect in the deep oceans, represented by the difference in mass between the water and an equal volume of rock, would produce negative anomalies as great as 500 milligals if not compensated by a corresponding mass-excess beneath the floor of the ocean. The observed anomalies are

in general positive, and average only one tenth to one twentieth of this magnitude. The large deviations from isostatic equilibrium which exist in the Meinesz belts may be considered as very exceptional features to be explained by stresses in the crust.

The results contradict the elliptical form of the equator deduced from continental gravity measurements by some geodesists, and favor a gravity formula which does not contain a longitude term.

Stokes (36) gave a theorem by means of which the form of the geoid may be deduced from gravity measurements, but in order to apply this theorem it is necessary to have a certain minimum density of gravity stations over the entire surface of the earth. Hunter (37) has recently made a thorough study of the number and distribution of stations needed for the application of the theorem of Stokes, and of the precision attainable in computing the distance of the geoid from the spheroid of reference at a given point. He concludes that a basic network of 1654 stations distributed uniformly over the surface of the earth would make it possible to carry through the computation with a probable error of $\pm$ 10.4 meters, while the addition of 84 suitably located local stations would reduce the probable error to $\pm$ 7.0 meters at a given point.

It is important that future gravity expeditions shall be planned with the need for this basic network in mind, and marine measurements are particularly important in this connection because many of the areas in which stations are needed are at sea.

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GRAVITY ANOMALIES AND ISLAND ARC STRUCTURE WITH PARTICULAR REFERENCE TO THE WEST INDIES

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MEINESZ's discovery of huge negative anomalies in the vicinity of island arcs is probably the most important contribution to knowledge of the nature of mountain building made in this century. In the space of this short paper a complete review cannot be made of his findings and of their interpretation, but the references given below will serve to direct the reader if he is inclined to go into the subject (1 to 8). When the significance of the huge anomalies and the structures which they indicate become appreciated, a radical change in some of our ideas on mechanics and processes of mountain building becomes necessary. Instead of pure speculation as to what happens in depth during deformation, certain concrete facts may now be substituted which definitely limit and control speculation to great advantage.

This symposium on geophysics and ocean basins consists of a group of papers dealing largely with new technique and apparatus for attacking geologic problems at sea; but this particular article is included as an example of the interpretation of data obtained from sonic soundings and gravity measurements. Specifically it deals first with general principles, and second, with structure and gravity anomalies in the West Indies. The data collected by four expeditions are made use of, these being the investigations of the Netherlands submarine K XIII by Meinesz (1926), U. S. Submarine S-21 by Meinesz and F. E. Wright (1928), U. S. Submarine S-48 by Meinesz, Hess, and T. T. Brown (1932), and U. S. Submarine *Barracuda* by M. Ewing, Hess, and A. J. Hoskinson

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(1937). A reasonably close gravity net is now available for the whole of the West Indian region, including a large number of land stations taken by the U. S. Coast and Geodetic Survey in the northern portion of the area. Through the invaluable coöperation of the Hydrographic Office of the U. S. Navy, many new sounding lines have been made recently in the Caribbean Sea. Much credit is due R. M. Field, whose keen foresight recognized from the beginning the immense importance of Meinesz's work at sea in relation to fundamental problems in structural geology, and through whose efforts the last two expeditions mentioned above were made possible.*

Facts Concerning Anomalies

1. *Distribution of Anomalies with Respect to Topography*

In most cases the strongly negative anomaly belt lies on the outer (convex) side of the main arc. Such is the case southwest of Java and Sumatra and southeast from Haiti around the Lesser Antilles to Trinidad. It may lie approximately over the axis of a long narrow ocean deep, as it does for example north of Puerto Rico. But most commonly it lies over, or a little to one side of, a ridge which in many cases is flanked by ocean troughs on either side. One of these troughs may be a great ocean deep. In the case of the Banda arc, where the negative strip lies along a ridge, the inner trough is a great deep, the Weber deep; and in the case south of Java where the anomalies are similarly located, the great deep, Java deep, lies on the outer side of the ridge. The ridge may project above sea level to form islands, as it does in the cases of the Banda arc, Timor, Nias, Barbados, Trinidad, and many others.

The belt of strongly negative anomalies is bordered on either side by complementary belts of positive anomalies which generally lie on topographic swells (geanticlines), the inner of which commonly emerges above sea level to form

* The complete report on the West Indies will be published shortly as the results of the cruise of the U. S. Submarine *Barracuda* (Navy-Geophysical Union Expedition).

the main arc. Examples of an emergent inner geanticline are Hispaniola, Puerto Rico, the Lesser Antilles, Java, and Sumatra. Christmas Island is one of the few examples of an emergent outer geanticline, though Aroe Island, part of New Guinea, and part of eastern Venezuela may also be cited.

2. *Size of Anomalies*

The maximum negative anomalies observed are over 200 milligals, and anomalies of 150 milligals are common. No anomalies of comparable size (regardless of sign) are known on the continents.

3. *Width and Length of Negative Anomaly Strips and Their Distribution*

The width of the negative strip, though variable, averages about 150 km. The strongly negative portion of the strip is commonly less than 50 km. wide. The negative strip in the East Indies is about 5000 miles long, but with further investigation it seems likely that it can be extended to the Marianne islands, continuing to east of Japan where the strip has been located, and thence northward to and along the Aleutians, making it very many times longer. A negative strip might be predicted to occur associated with the New Guinea-New Zealand arc, and another will almost certainly be present around the Cape Horn, South Georgia-Antarctica island arc. Details of the negative strip in the West Indian arc will be dealt with in this paper. Indications of a negative strip in the Mediterranean from the Gulf of Taranto and southwestward on the Alpine trend have been found and a similar belt perhaps may be found on the Himalayan trend along the north shore of the Arabian Sea. (It seems likely that negative strips will be found only in the arcs mentioned above and nowhere else.)

In no case are two parallel strongly negative strips known, but in every case it appears that the negative strip is a single continuous band.

4. *Location of Volcanoes with Respect to the Negative Anomaly Strip*

Volcanoes lie on the concave side of the negative strip and about 160 km. away from the axis of strip. They are probably located on radial fractures roughly perpendicular to the negative strip. Groups of two or three volcanic centers, one behind the other along radial lines, may be noted on Java for example.

5. *Serpentinized Peridotite Intrusions and the Anomaly Belt*

Serpentinized peridotite intrusions occur along or very close to the negative strip, cutting rocks older than the first great deformation, which deformation was probably responsible for the first development of the negative strip. They seem to be intruded during the deformation.

Explanation of the Negative Anomaly Strip

The anomalies of the negative strip are so large and of such a local nature that many of the explanations possible for small or regional anomalies can be eliminated. Thus the sum of the errors which may be introduced by observational inaccuracies, imperfections in topographic corrections and gravity formulæ, or incorrect assumptions as to the type of isostatic compensation, cannot make up more than a small fraction of such anomalies as these. There can be no doubt that the major portion of the anomalies present in the negative strip is due to an abnormal mass distribution in the Earth's crust below the strip, and probably to local departure from isostatic equilibrium.

Meinesz's explanation for the anomalies of the negative strip is that the Earth's crust buckles downward in a huge vertical isoclinal fold, and thus the light material of the upper crust might extend downward to a depth of 40 to 60 km. This would be sufficient to give the observed anomalies if the lower portion of the downbuckle had a specific gravity deficiency of about 0.3. No other adequate explanation has yet been advanced (Fig. 1).

Application of the theory to the West Indian region results in the coördination of many geologic facts previously merely a collection of observations with no apparent relationship to one another. So successful is the Meinesz theory in the West Indies, both in predicting relationships which are to be expected (and have since in a number of cases been found) and in joining together in a single structural entity observations which formerly fit into no definite pattern, that the writer has become completely convinced of the soundness of

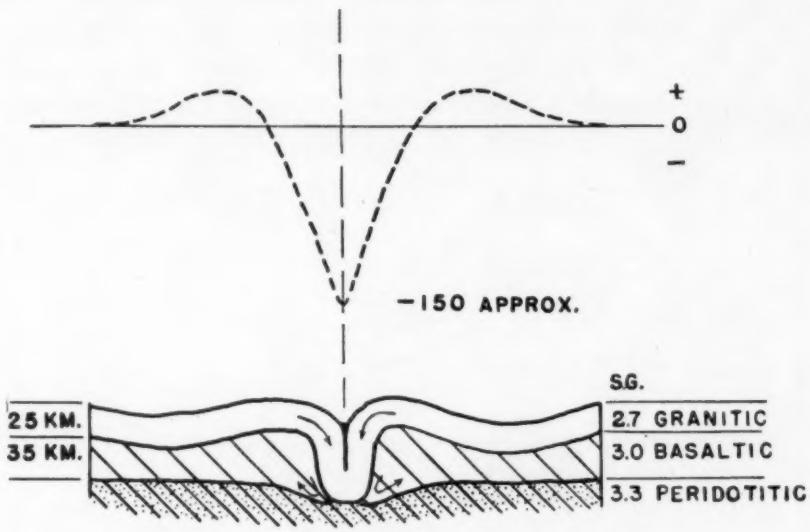


FIG. 1. Crustal buckle, specific gravity distribution and resultant anomaly curve.

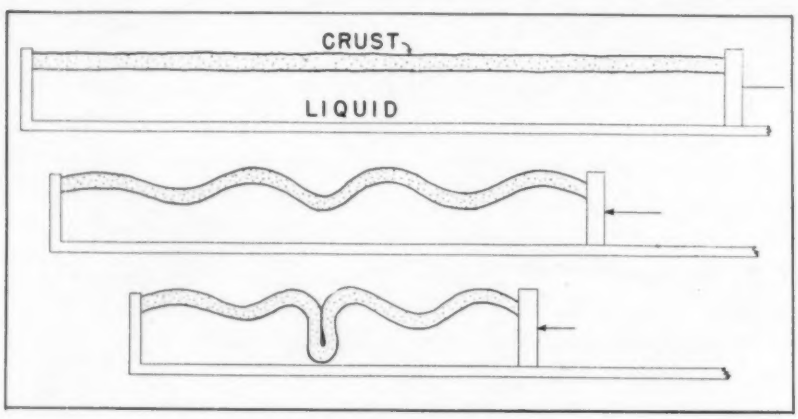
the theory in general, though in detail it may need some modification. He believes, as Meinesz originally postulated, that only the upper 25 km. of the crust buckles, and the material beneath that deforms by flowage laterally. This idea is a corollary of the writer's concept of the formation and intrusion of peridotites in the region of the tectogene (23).* It seems very likely that the crust becomes gradually weaker with depth as temperature increases, until finally a zone of practically no strength is reached at the base of the crust.

* Tectogene = crustal downbuckle, Kuenen (2).

Thus while the upper part of the crust may have sufficient strength to be buckled downward, the lower part would certainly have less strength and might reasonably be considered to deform by flowage. Inasmuch as the anomalies indicate a disturbing factor about 50 km. wide, the division between crust which has buckled and crust which has flowed may well be placed at 25 km. depth (the 50 km. width consists of the two limbs of the isoclinal downfold each 25 km. thick).

Kuenen's Experiments

Kuenen's (2) analytical examination and experimental test of Meinesz's theory offers strong support. The experiments performed by Kuenen are of particular validity because



KUENEN'S EXPERIMENTS

Fig. 2.

two factors have been taken into consideration which are commonly neglected in experimentation with geologic models. The first is that the strength of materials used has been chosen so as to be of the right order of magnitude for the scale of the model; and second, the crust of the model has been floated on liquid paraffin to supply the zone of no strength beneath the crust.

The experiments resulted in, first, the bending of the crust of the model into a regular series of sine curve folds, and finally into the buckling downwards of one syncline (Fig. 2).

(Two adjacent synclines buckled downward in a few experiments, and then moved together and coalesced into a double structure. No analogy to this has been found in island arcs.) In no case does an anticline buckle upwards. Once a syncline has buckled, further compression merely deepens that syncline, the "crust" coming up to that syncline and then running downwards over "rolling hinges."

In another set of experiments, Kuenen covered the crust with a very weak layer to represent a cover of sediments. In the first stages of deformation, the sediments are carried along on the competent crust; but when the crust downbuckles, the weak sediments are squeezed out of the core of

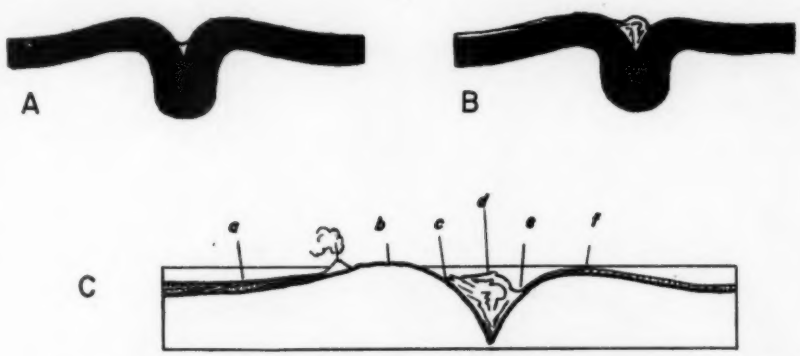


FIG. 3. *A*. Buckling of the crust with little or no incompetent cover. The anomalies lie over an ocean deep. *B*. Incompetent cover present and weak material is squeezed up in geotectoclinal zone. Anomalies lie over a ridge. *C*. The same in detail. *a* = geosyncline, *b* = emergent inner geanticline, *c* = ocean trough of moderate depth, *d* = geotectocline—upsqueezed zone, *e* = great ocean deep, *f* = submerged outer geanticline.

the tectogene like toothpaste out of a tube, forming alpine types of structure (Figs. 3, 4, 5). As will be seen later, structures of this upsqueezed type are found in connection with the negative strip in the West Indies.

THEORETICAL IMPLICATIONS OF THE MOUNTAIN-BUILDING BY DOWNBUCKLING

A further modification of Kuenen's experiments might be considered. In both the East and West Indies, according to

the writer's concept, a second great deformation has occurred a considerable time after the first one, during which the tectogene originally was developed. In the interval between the first and second great deformations, one or both of the geanticlines on either side of the tectogene may have emerged above sea level. Erosion of these emergent portions, plus a

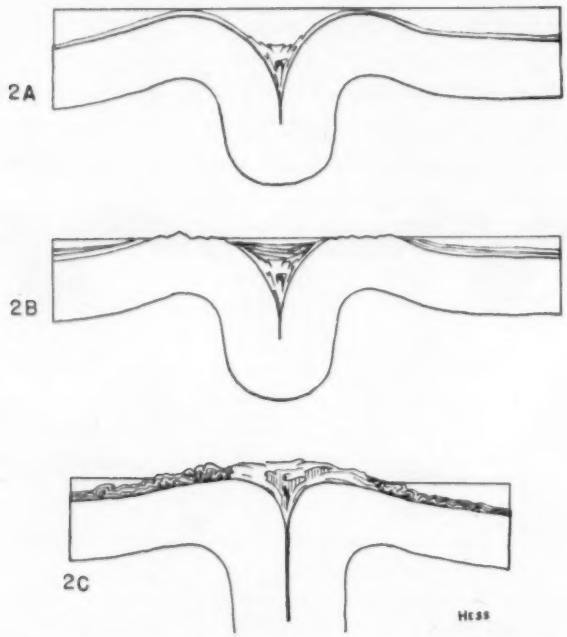


FIG. 4. 2*A* similar to *B* of figure 3 but whole structure here is submerged.
2*B*. Structure emerges and erosion and sedimentation take place.
2*C*. A second great deformation forces the tectogene down further. The old deformed sediments of the original geotectocline are pushed up further and the younger interdeformational sediments are strongly deformed by the jaw crusher effect as the two limbs of the tectogene come in and go down over rolling hinges. The younger sediments on the geanticlines are much folded when they come up against the bulwark formed by the geotectocline.

great contribution of volcanics from the concave side of the arc, may deposit great thicknesses of material in "geosynclines" within the inner geanticline, and perhaps also outside of an outer geanticline, as well as in the central basin over the tectogene itself. This basin over the tectogene will

henceforth be called the "geotectocline" because of its different structural behaviour and in many cases its different type of sedimentary sequence than that which occurs in a geosyncline as the term is generally used today.* The second

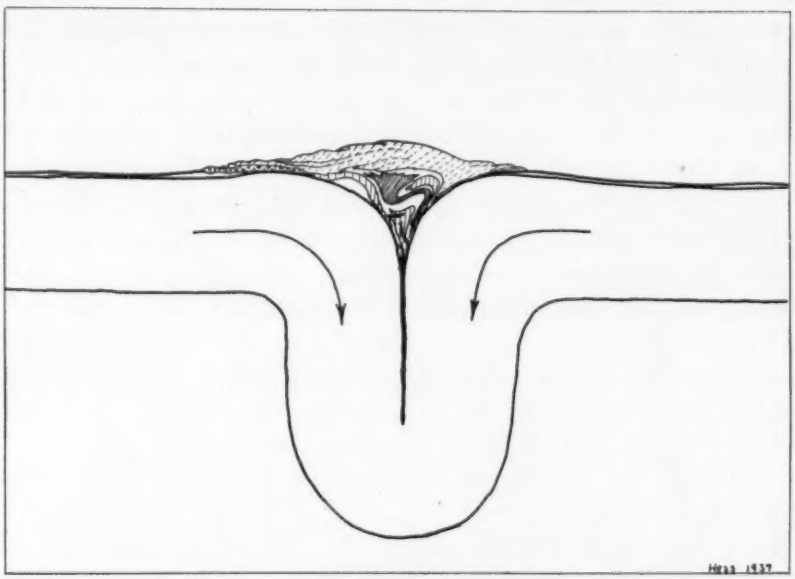


FIG. 5. General section of the Alps superimposed on the tectogene. Both features drawn to the same scale with no vertical exaggeration.

deformation will deform very intensely the material of the geotectocline. Strong folding and perhaps thrusting of the interdeformational sediments,† if deposited, will occur, and probably further upthrusting of material originally squeezed

* I hesitate to introduce a new term, but this one seems necessary unless a long descriptive phrase be written out each time this structural unit is considered. The term "geosyncline" itself is difficult to use because it has been defined in several different ways some of which are expressed in terms of hypothetical structural concepts and hypothetical conditions of sedimentation which probably have no actual counterparts in nature. In this paper the term geosyncline is used broadly to signify an *elongate gently downwarped trough* of large size (at least tens of miles and perhaps hundreds of miles or more in length). The definition specifically includes no implication as to the manner of its warping. Sediments may or may not be contained in it; but, if present, the nature of the sediments is not specified.

† Sediments deposited between two periods of deformation.

out of the tectogene, if present, will take place. This happens because the material in the geotectocline is pinched between a sort of jaw-crusher as the main crust moves toward the tectogene and down over its rolling hinges. Furthermore, the material which may be on the geanticlines or in the adjacent geosynclines on the sides (or side) away from the geotectocline will be carried forward toward the geotectocline. This material may then impinge against the upsqueezed mass in the geotectocline. Upon coming against this bulwark, the weak upper part may be literally scraped off the main crust as it rides forward and down into the tectogene. This is particularly true if very incompetent horizons, such as salt beds or argillaceous sediments are contained in it. The result will be that the cover will be thrown into folds and perhaps develop a schuppen structure as its forward progress is stopped by the bulwark and the main crust under-rides or in reality underthrusts it. The Juras can perhaps be cited as an example of such structure (Figs. 3 and 4).

Though the above is presented as speculation, it was arrived at largely by consideration of known structures in folded mountain systems and built up from generalizations concerning these systems which most geologists would today accept. These generalizations were then fitted to the theory of downbuckling. The narrow central area of intense deformation and metamorphism so common in intensely folded mountain systems becomes the geotectoclinal zone. The broad areas generally characterized by open-folding and in many cases by great thickness of sediments become the adjacent geosynclinal areas. This is the tectonic zone in which a large proportion of the world's great oil fields are found. Thus in the West Indies the geotectoclinal zone may be seen in the metamorphosed and much deformed sedimentary rocks of the North Range of Trinidad and its continuation in the coast ranges of northeastern Venezuela; and the folded geosynclinal zone of thick sedimentary sequences (younger than the first deformation in large part) may be found to the south in areas now being exploited for

oil. In the East Indies the geotectoclinal zone is found on Nias, Timor, Tenimber, Ceram, etc.; and the folded geosynclinal zone is represented by the great oil field areas of Java, Sumatra, etc. (Umbgrove's poly- and idio-geosynclinal areas respectively (1)).*

Let us now turn from general theory and speculation to the more concrete topic of actual relations in the West Indies.

The West Indies

Gravity Field (Plate I)

The most significant feature of the gravity field is the well defined negative strip which extends from the eastern end of Cuba completely around the arc to Trinidad where it probably crosses that island, the Gulf of Paria, Northeastern Venezuela just south of the Coast Range, hence northwestward to sea around the north side of the Dutch islands, and finally southward into Colombia. The portion between Trinidad and the Dutch islands has not been investigated, but almost certainly the anomalies will be found there. Meinesz is probably taking some stations in that area at present in connection with tests he is making.

The anomaly values along the axis of the negative strip from Cuba to Trinidad commonly reach − 150 milligals. The largest anomaly is − 183 over the Brownson Trough north of Puerto Rico. Here and north of Hispaniola the axis of the strip is practically coincident with the axis of the trough. Over the sharp flexures in the negative strip, both at the north end of the Lesser Antilles northeast of the Virgin Islands, and at the south end in the vicinity of Trinidad, the anomalies are somewhat smaller, being under − 100 milligals.

* Umbgrove's and Kuenen's analysis of the East Indies (1) have been most helpful in working out the West Indies and served as a solid base from which to begin. On the whole our conclusions agree remarkably well, only differing at a few points. In dealing with such complex sets of data involving much inference it is surprising and also pleasing that we do agree so well. It is also to be expected that the differences in evidence found in the East and West Indies and differences in viewpoint and attack of the investigators are bound to lead to some divergences of opinion.

Perhaps this is due to a lateral stretching of the tectogene where the sharp bends in the strip occur.

The Brownson Trough, previously known north of Puerto Rico and the Virgin Islands, curves around parallel to the Lesser Antilles and extends almost as far south as latitude 17°. Southward from this latitude a ridge appears along an extension of the same axis. Barbados lies on this ridge, and the negative strip continues along the ridge just as it did along the deep. This is interesting when Kuenen's experiments are taken into consideration, for certainly the topography and gravity anomalies north of Puerto Rico strongly suggest the type of structure shown in Fig. 3a, whereas a section from the Lesser Antilles through Barbados indicates the structural conditions of Fig. 3c. In the East Indies the negative strip generally does not lie over the deep but along a ridge which may be flanked on one or both sides by troughs. Presumably most of the East Indian negative strip corresponds to the Barbados type of structure.

From latitude 19° to latitude 15°, the anomaly axis lies to the west of the axis of the deep at the north end, and west of the axis of the ridge at the south end. The anomaly profiles are somewhat asymmetrical, being steeper on the west side of the axis. This may mean a slight eastward overturning of the tectogene.

The strongly negative strip terminates at the eastern end of Cuba. A belt of smaller negative anomalies, however, does continue westward with its axis approximately over the northern half of Cuba. The writer believes it very probable that the negative strip did at one time extend the length of Cuba and thence southwestward to and across Gautemala. The writer will return to a discussion of this point later.

Positive anomalies are found in bands on either side of the negative strip. The crests of the positive bands lie about 150 km. on either side of the axis of the negative strip, though in a few places they may come as close as 100 km. or as far away as 200 km. The positive anomalies of these bands generally range from + 20 to + 40 in the West Indies.

An exceptionally large anomaly is found at Curacoa, however, + 80. The analogous positive anomaly bands in the East Indies generally show much higher values, many being over + 100, although the negative values found in the East Indian negative strip are about the same as those of the West Indies, or a little smaller on the average.

Outside of the arc a faint suggestion of small undulations exists parallel to the negative strip, both in gravity field and topography. The distance from crest to crest of these undulations seems to be about 250 km.

In the basins of the Caribbean and Gulf of Mexico the anomalies are generally positive. As a rule, the deeper the water for a given basin, the more strongly positive are the anomalies. The profile made by the S-48 from Jamaica to the west end of Cuba shows the relationship most clearly. Here the anomaly profile drawn to an appropriate scale over the sounding profile results in the one becoming an almost perfect mirror image of the other. Enclosed basins in other parts of the world show similar relations. The reason for this relationship is not apparent, and no adequate explanation can be offered at present. Meinesz's suggestion of downward convection currents below the crust to explain the positive anomalies in the oceans does not seem applicable here, because such a process going on at considerable depth does not seem consistent with the very close correlation of anomalies and topography.

Serpentinized Peridotite Intrusions (Fig. 6)

In the East and West Indies, as well as in other island arcs of this type, a belt of serpentinized peridotite is always present in the most intensely deformed zone. In the East and West Indies the serpentines may be seen to follow closely the negative strip except where younger sediments and volcanics hide them from view. The writer has made a fairly exhaustive study of this particular type of intrusion (21 to 23). The conclusions arrived at are that the serpentinized peridotites of the type found in island arcs and in the

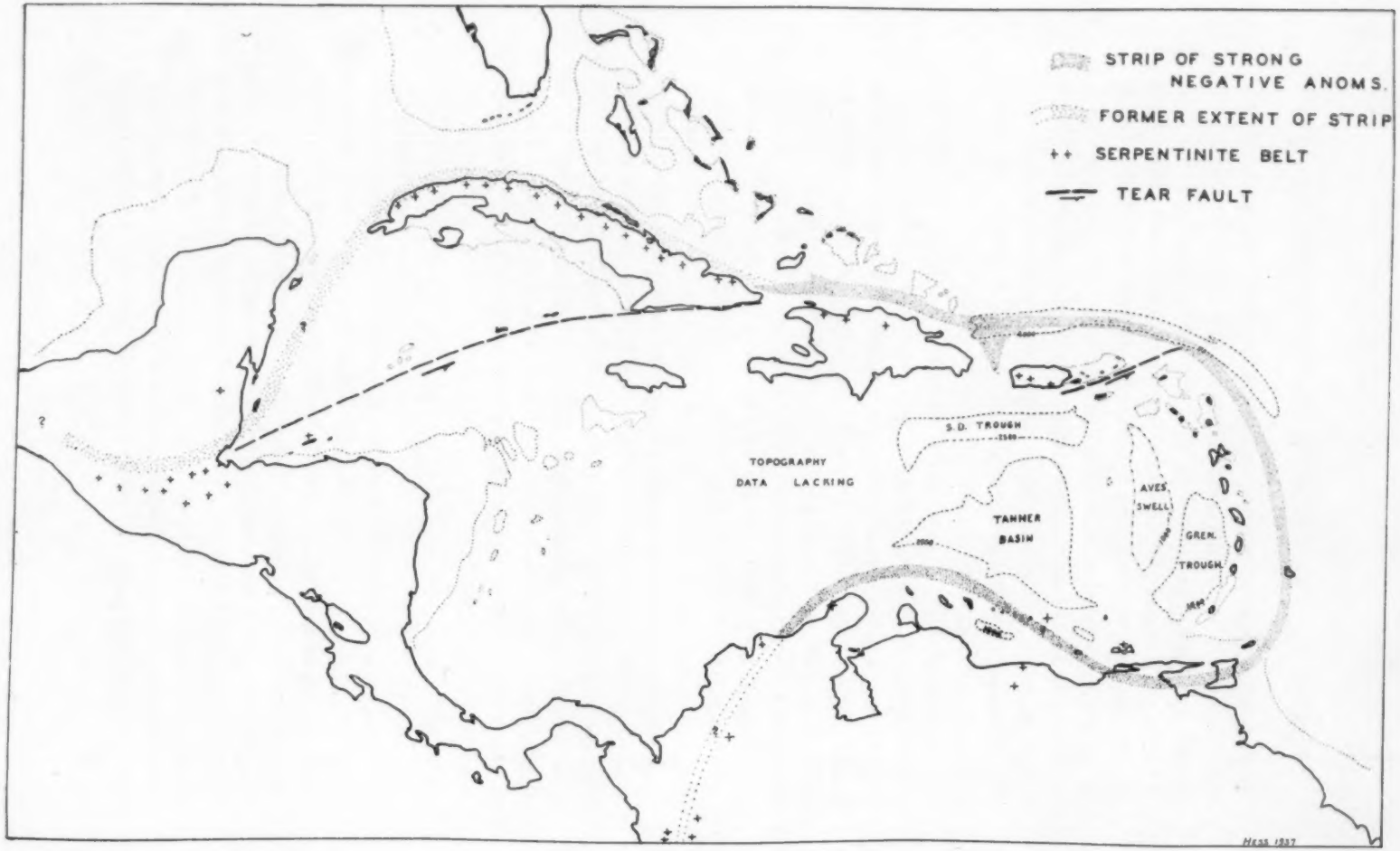

STRIP OF STRONG NEGATIVE ANOMS.
FORMER EXTENT OF STRIP
++ SERPENTINITE BELT
TEAR FAULT
S.D. TROUGH
TOPOGRAPHY DATA LACKING
AVES SWELL
TANNER BASIN
GREN. TROUGH
HESS 1937

FIG. 6.

geotectoclinal zone of all mountain systems represent an ultramafic magma which is intruded during the first great deformation when the tectogene is formed. Furthermore, they apparently never occur under any other conditions than in connection with such highly deformed zones. They are extremely useful indicators of the position of the geotectoclinal zone in old, now deeply eroded, mountain systems; and also are most helpful in accurately dating the time of deformation, and extending this dating for the whole length of a given serpentine belt. A second deformation, shoving the same tectogene down again, does not seem to produce a second period of intrusion of this magma.

In the West Indies a great serpentine belt extends east-west across Guatemala, the whole length of Cuba north of the central axis, through northern Hispaniola, and across Puerto Rico. It is not found in the Lesser Antilles, because if it were present it would probably lie a little to the east under water; but if some of the marginal intrusions were present in these islands, they would be covered by the younger sediments and volcanics which are all that outcrop on them. The serpentines reappear in the island of Margarita, and are present on Orchila and El Roque, and again at Cape Vela on the Goajira peninsula; hence they apparently bend southward to form the great serpentine belt of the Cordillera Central of Colombia.

The serpentines intrude Upper Cretaceous rocks for the most part. But in Cuba they are probably younger than certain Lower Eocene beds, and definitely older than the Upper Eocene on that island. There is much evidence of intrusion during deformation. The most probable date of intrusion for the whole belt is approximately at the end of Middle Eocene.

These intrusions may be made use of in two connections in the West Indies. First, they indicate the position of the negative strip in the unsurveyed stretch between Trinidad and the station north of Bonaire where the strip was again picked up. Second, they clearly show that the negative

strip once continued along the north coast of Cuba, and thence to Guatemala and westward, but it has since largely disappeared. They also show that the negative strip in the southern Caribbean joins with the Cordillera Central and once extended down along it, though today it has probably disappeared just as is the case in Cuba.

Age of the Negative Strip

Though many geologists who have not worked in the West Indies have a tendency when writing about that area to place a great deformation at the end of the Cretaceous, there is fair agreement among those who have studied the region in recent years that the great deformation came within the Tertiary. It apparently should be placed before the widespread Upper Eocene deposits and after certain Lower and Middle Eocene sediments. The end of Cretaceous was in many places marked by emergence, so that a disconformity may be present between it and the Eocene; but so far as the author is aware, no folding occurs at this time in the West Indies.

The most intense zone of deformation of earliest Tertiary and Cretaceous rocks follows the present negative strip (and its former extensions as suggested above). Portions of the same zone were strongly deformed again in late Miocene and Pliocene time.* The serpentines, as has been mentioned, accompany only the Eocene deformation. The conclusion is that the negative strip was formed during the Eocene deformation, but has been rejuvenated to some extent by further shoving down of the tectogene during the late Tertiary deformation.

Great Faults of the West Indies and Their Relation to the Anomalies

The greatest fault in the West Indian area is that which lies along the northern scarp of the Bartlett Trough from

* Another deformation about middle Oligocene in age may be noted in Haiti and northern South America.

Guatemala to the Windward passage, and bounds the southern coast of eastern Cuba. This fault is still active today, but some consider it a feature which has been active since great antiquity. With this the writer cannot agree. He believes it was formed during the late Miocene deformation. Hill (13) says that the conglomerates of the Richmond beds (early Eocene) in Jamaica indicate land at that time to the northeast (now the eastern part of the trough). The now isolated islands on the borders of the trough, Swan Island and the Caymans, have Tertiary limestones containing such terrigenous material as quartz and mica. Furthermore, it is not likely that the flat-lying Oligocene and Miocene limestones of the Caymans, which have in part been correlated with Jamaica, were laid down at the very brink of a great fault trough which now separates Jamaica from those islands.

The writer believes that the fault along the north side of the Bartlett Trough is one along which horizontal movement took place during the later Tertiary deformation. The south side moved east; the area south of the fault was thus relieved of compression and became complexly block faulted. The area north of the fault, Cuba, suffered only a moderate amount of late Tertiary deformation, whereas Haiti, which was not so relieved, became very intensely folded in Miocene and Pliocene time. This is the cause of the chief difference in the late Tertiary geologic histories of Cuba and Haiti. Also it explains the abrupt disappearance of the negative strip at the east end of Cuba and its present absence from the whole length of Cuba. Presumably movement along the fault took up the stress of the late Tertiary deformation, so that the tectogene was not rejuvenated north of the fault and actually pressure has been so much relieved that the tectogene under Cuba has largely disappeared by lateral spreading, isostatic uplift, or both.

A smaller fault on the north side of Anegada passage, south of the Virgin Islands, which probably is of the same type, has had a similar though less striking effect on the area to the north of it. The late Tertiary rocks of Puerto Rico,

according to Myerhoff, are but slightly deformed. The diminished values of the negative anomalies of the strip north of the Virgin Islands may also be an effect of the Anegada fault, though the writer has suggested an alternative explanation above.

An east-west fault is suggested by the topography just north of Barbuda.

On the east side of the negative strip a great fault scarp 6,000 feet high (south side up) was located by sonic soundings east of Guadeloupe near longitude 57°. This apparently also extends east-west.

A number of east-west faults were mapped by H. Hanna on the island of Barbados.

Great faults probably bound the block of metamorphosed rocks comprising the north range of Trinidad and its extension into northeastern Venezuela according to Kugler. These ranges may represent up-squeezed blocks of metamorphosed Cretaceous sediments in the geotectoclinal zone.

All of these faults, except the last mentioned, run up to and probably terminate in the geotectoclinal zone. It seems to the speaker they were formed by adjustments of the crust as it moved up to the tectogene and down into it. Relative differential horizontal movements of one sector as compared to an adjacent one would be expected to take place as the crustal downfold formed. Most of these adjustments, which are now discernible, seem to be related to the second great deformation.

If we postulate a downbuckling of the upper crust to a depth of 60 km. (or perhaps more if several deformations of the same tectogene take place) along a sinuous curve such as presented by the East or West Indian negative strips, great horizontal adjustments must take place in the crust, both inside and outside of the arcs. Horizontal movements of the order of magnitude of 50 km. along such faults as the Bartlett Trough fault are not only likely, but inevitable, if the crust is to adjust itself to a curved downbuckle of this size. Thus it is not inconceivable that Haiti has moved N.N.E. 50 km. or

more, and once stood below and adjacent to Cuba, or that the portion of the crust now forming the Lesser Antilles geanticline once stood far to the west of its present location, so that it formed a continuous chain with the southeastward trending folds to be found in the Cretaceous rocks of Puerto Rico, which now strike into the Caribbean, where no trace of them is discernible on the sea floor.

Structures of the Geotectoclinal Zone

The small area of outcrops of older rocks in Barbados presents an extremely interesting picture. Barbados lies almost on the axis of the negative strip and typical geotectoclinal conditions should obtain there. The writer offers the following interpretation of the history of Barbados. The Oceanic series, probably deep-water sediments, are moderately deformed. They lie above the very intensely deformed Scotland series, which are shallow water or perhaps in part terrestrial sediments of quartzose character, no doubt derived from a once emergent geanticline to the west. The lower part of the Oceanic series is Upper Eocene in age according to Kugler, and the Scotland series very slightly older. Between these two formations is found what has been described as a "mud flow," containing huge blocks of sandstone in a silty matrix. But to the writer this breccia bears a strong resemblance to the outcrops of the great submarine landslide which he has seen in the city of Quebec (see Collet, Bailey, and Field, 14). Furthermore, since they lie on the junction between shallow water or terrestrial sediments below and deep sea sediments above, with practically no time interval between them, this hypothesis becomes even more likely. The following sequence of events is tentatively suggested. In the interval of time between the deposition of the Scotland series and the Oceanics, the great deformation took place forming the tectogene. The Scotland series were squeezed up out of the core of the tectogene and thus intensely deformed, but the downward movement forming the tectogene left the tops of these folds at the bottom of a deep steep-sided

ocean trough. Silt and blocks of the Scotland series rocks slid down the steep sides covering portions of the bottom of the basin. This was followed immediately by the beginning of deposition of the deep water sediments—the Oceanics. For a long period of time the deposition continued. It is not known what the upper age limit of the Oceanics is. Sedimentation may have continued into the Miocene. In the Miocene deformation the Oceanics were folded and the whole sequence squeezed further up out of the tectogene again, approaching the surface of the sea or actually emerging. Some marine sediments and coral reef formations follow, accompanied by further uplift, possibly as a result of isostatic adjustment. Today the island emerges from the sea to a height of 1,100 feet and the coral reef cover has been stripped off the northeastern portion.

In general thrust faulting and overturning of anticlines occurs in both directions outward from the axis of the geotectoclinal zone. In most cases thrusting and overturning is much more extensive on one side of the axis than on the other (the Alps, for example). In northeastern Venezuela the folding becomes much more intense, proceeding from south to north toward the Coast Range; and as that range is approached thrust faults with southward displacement may be found (see Hedberg and Kamen Kaye, 15, 16). In Trinidad south of the North Range, overturning and thrust faulting is dominantly southward. In the metamorphosed sediments of the North Range itself, the overturning is in the opposite direction toward the north, and small thrust faults with northward displacement may be seen in these rocks. Very close to the south side of the North Range, late Tertiary sediments may be observed dipping in toward the range. The junction between the late Tertiary sediments and Cretaceous metamorphics of the North Range here must be a steep fault. It appears that the rocks of the North Range have been thrust steeply upward and represent a slice of rocks from deep in the geotectoclinal zone. This movement may represent an isostatic adjustment since the last deforma-

tion (in as much as the range is nearly over the axis of the tectogene), taking place along older steeply dipping fault planes related to the Mio-Pliocene deformation.

Trinidad is also interesting in that it presents a very close resemblance stratigraphically and structurally to Barbados. The Mount Moriah formation, with a great boulder bed and block conglomerate at its base, is equivalent and analogous to the Oceanic series of Barbados with its basal "mud flow." No doubt Trinidad in this epoch has a history much the same as that already outlined for Barbados.

Again in Curacoa, on the south side of the geotectoclinal zone, Molengraaf (17) described the folding as decreasing in intensity southward, and the overturning is toward the south.

Along the north coast of Cuba the overturning of folds and thrusting seems to be dominantly northward; but on the south coast in Pinar del Rio and probably also in the Trinidad mountains, the thrusting and overturning are in the opposite direction. The axis of the geotectoclinal zone runs between the two.

Volcanic Activity

Volcanic activity at present is limited to the smoothly curving arc from Saba on the north to Grenada on the south. This arc terminates abruptly a few miles northwest of Saba and a few miles southwest of Grenada. This arc does not represent the crest of the inner geanticline. The structural crest generally lies thirty or forty miles to the east of it. At the north end of the arc islands, largely made up of sedimentary rocks, occur along the crest, but the southern half of the crest is covered by water.

The volcanic arc is probably underlain by Cretaceous rocks, such as those in the Virgin Islands. Lacroix's (18) description of xenoliths of the basement rocks brought up in the volcanics include gabbros, diorites, diabases, metamorphosed limestones, and cordierite rocks, no doubt metamorphosed equivalents of argillaceous sediments. Almost all

of the islands, though dominantly made up of volcanic material, do have small amounts of sediments of various ages.

At the end of the Cretaceous, volcanic activity was much more extensive, however, and apparently extended for the whole length of the present geotectoclinal zone. Thus great masses of volcanic material are present in the late Cretaceous sequence of all the Greater Antilles as well as in those of the Dutch islands north of Venezuela. Where erosion has cut deeply enough, quartz diorite and granodiorite intrusives may be exposed, no doubt the intrusive equivalents of the dominantly andesitic volcanics. These are of the same type chemically as the great series of Andean intrusions.

Sonic soundings show a series of peaks on the western flank of Aves Swell parallel to the Lesser Antilles arc and 250 km. west of it. Aves Island is the only one which projects above sea level. Nothing is known concerning the nature of this rock. Profiles across the peaks strongly suggest submerged volcanoes. The lack of seismic activity along the greater part of this swell in the vicinity of the peaks suggests that if they are volcanoes they are extinct. One peak a few miles northwest of Aves has, however, been the locus of three earthquakes. These are the only ones recorded for the whole length of the swell.

Seismic Activity

Earthquake shocks of low intensity are very frequent along the Lesser Antilles arc and may be directly correlated for the most part with activities of the volcanoes.

In a general way it may be said that there is a correlation between seismic activity and the negative anomaly strip. It is not, however, a direct relationship. Comparatively few strong earthquakes have their epicenters on the strip. From the Virgin Islands to Barbados, for example, not a single earthquake has been recorded on the strip. Again, north of Hispaniola no earthquake epicenters are found on the negative anomaly belt. In the southern Caribbean no epicenters lie on the strip for its whole length, from where it probably goes

to sea in northeastern Venezuela to Colombia (as Captain Heck points out in the discussion). A strong earthquake, however, did occur a few years ago just where the strip presumably leaves the mainland of Venezuela, striking to the northeast (Cumana earthquake). The only earthquake epicenters recorded on the strip or very close to it occur from Barbados to Trinidad and in the Brownson Trough. The only conclusion which the writer can draw from these data is that the tectogene is at present in a rather plastic state and deforms by flowage rather than by the sharp displacements along fractures which would produce earthquakes.

Earthquakes are numerous and commonly of major strength along the fault zones and probable fault zones which run into the tectogene. In this zone we are dealing with normal crust which, unlike the now heated up crust deeper in the tectogene, reacts to stress in many cases by fracturing. It is these readjustments (mostly in the inner, concave side of the arc, but also present on the outer side) caused by differential movements of various sectors of the crust as they move toward the tectogene which apparently produce most of the earthquakes. In this sense there is a correlation between seismic activity and the belt of strongly negative anomalies.

The Puerto Rico-Virgin Island area close to the Anegada fault is an area of great seismic activity. Seven earthquake epicenters are located in the Bartlett Trough, and quite a few more in the immediate vicinity, particularly in Jamaica.

The west side of Central America northward into southern Mexico is a region of most intense seismic activity, and in part an area of great volcanic activity. These phenomena, however, do not seem to be related to the major structures of the island arc, but are of more recent origin and probably are to be correlated with a northwesterly trending fault zone diagonal to regional structure.

As Captain Heck points out,* the earthquake belt in northern South America departs from the negative anomaly strip and swings westward from Trinidad across Venezuela,

* See discussion in this symposium.

and then southward in western Venezuela and eastern Colombia with the Eastern Cordillera of the Andes.

It is interesting to note that the area north of the great Bartlett Trough fault is completely devoid of earthquake epicenters. All of Cuba, Florida, and the Bahamas, are free of earthquake epicenters (except for a single center near the eastern margin of the area in the channel north of the eastern end of Cuba). This area seems to be one of unusual quiescence at present. The reason is the same as that given for the general lack of strong deformation in Cuba at the time of intense folding in Haiti and elsewhere in the West Indies in late Tertiary time. The release of stress from this area by movements along the Bartlett fault must be very effective. Indirectly it offers proof of the Miocene age of the Bartlett Trough, for if the fault were there during the great Eocene deformation, the Cuban area would not have been so intensely deformed.

Conclusion

At least one positive conclusion can be drawn from the geophysical and geological study of the West Indies: that is, a settlement of the long standing controversy as to whether the Lesser Antilles represent a structural connection between the east-west ranges of the Greater Antilles and the east-west ranges of Venezuela, which are a continuation of the Andean chain, or whether the Antillean ranges and Venezuelan ranges both continue eastward in structures beneath the ocean, perhaps to join the Alpine chains of Europe and northern Africa as some European geologists suggest. The Lesser Antilles are on a geanticline and do represent the structural connection between the Venezuela chain and the Greater Antilles, as Suess originally postulated, and the contrary interpretation may now be discarded.

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Notes on Plates (in pocket).

PLATE I. Tectonic Map.

Most of data on tectonic trends on land areas taken from the literature. Submerged structures suggested by topography, gravity anomalies, or both. Topography from Hydrographic Office charts, the Barracuda Expedition and recent soundings taken by the U. S. Navy. With certain exceptions, sufficient soundings are available to make all major features of the topography show reasonably accurate, though no doubt more soundings would add much to the detail. The exceptions to the above statement are the following: (1) North of the Gulf of Maracaibo and hence northwestward to the south end of Beata Ridge; (2) A large portion of the area included on this chart of the Atlantic Ocean outside the Lesser Antilles.

PLATE II. Provisional Isostatic Gravity Anomaly Chart.

The gravity anomalies taken by the four expeditions to the West Indies, as well as a large number of land stations taken by the U. S. Coast and Geodetic Survey parties, furnish the basis for the contours. The stations at sea are widely spaced. If a closer net of stations were taken, no doubt a much greater complexity in detail would be found, just as has been found where the stations are closely spaced in Cuba. Nevertheless, it is unlikely that the main features of the gravity field as shown would be materially changed. The anomaly field from eastern Venezuela to north of Bonaire is doubtful. The probable former extension of the negative anomaly strip into Colombia is indicated only by the geology. A narrow east-west positive area crossing central Puerto Rico has been omitted.

THE ROLE OF EARTHQUAKES AND THE SEISMIC METHOD IN SUBMARINE GEOLOGY

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THE thought of an effective attack on the problems of submarine geology is rather appalling in view of the vast extent of the oceans. To bring the problem within reasonable bounds there must be limited objectives. The problem is so new that even these cannot be attained without the continuing development of fundamental methods. There is need for careful selection of areas which would be suitable both for the development of methods and also for furnishing valuable results when attacked by these methods. My purpose is to show that seismic considerations will prove a useful guide in the selection of these areas.

Earthquakes are associated with mountain structures whether of the continents or of the island arcs and with their associated deep ocean troughs. They are known to occur along the great rises or ridges in the Pacific, Indian and Atlantic Oceans and the inference is reasonable that these are mountain structures, especially as earthquake epicenters are relatively rare in the adjoining ocean basins. If these rises are mountain structures, their surface expression is probably very different from that of continental mountains. Even if they may have once been above the sea, they have undoubtedly been submerged for vast periods when no transfer of material by erosion and deposition could take place.

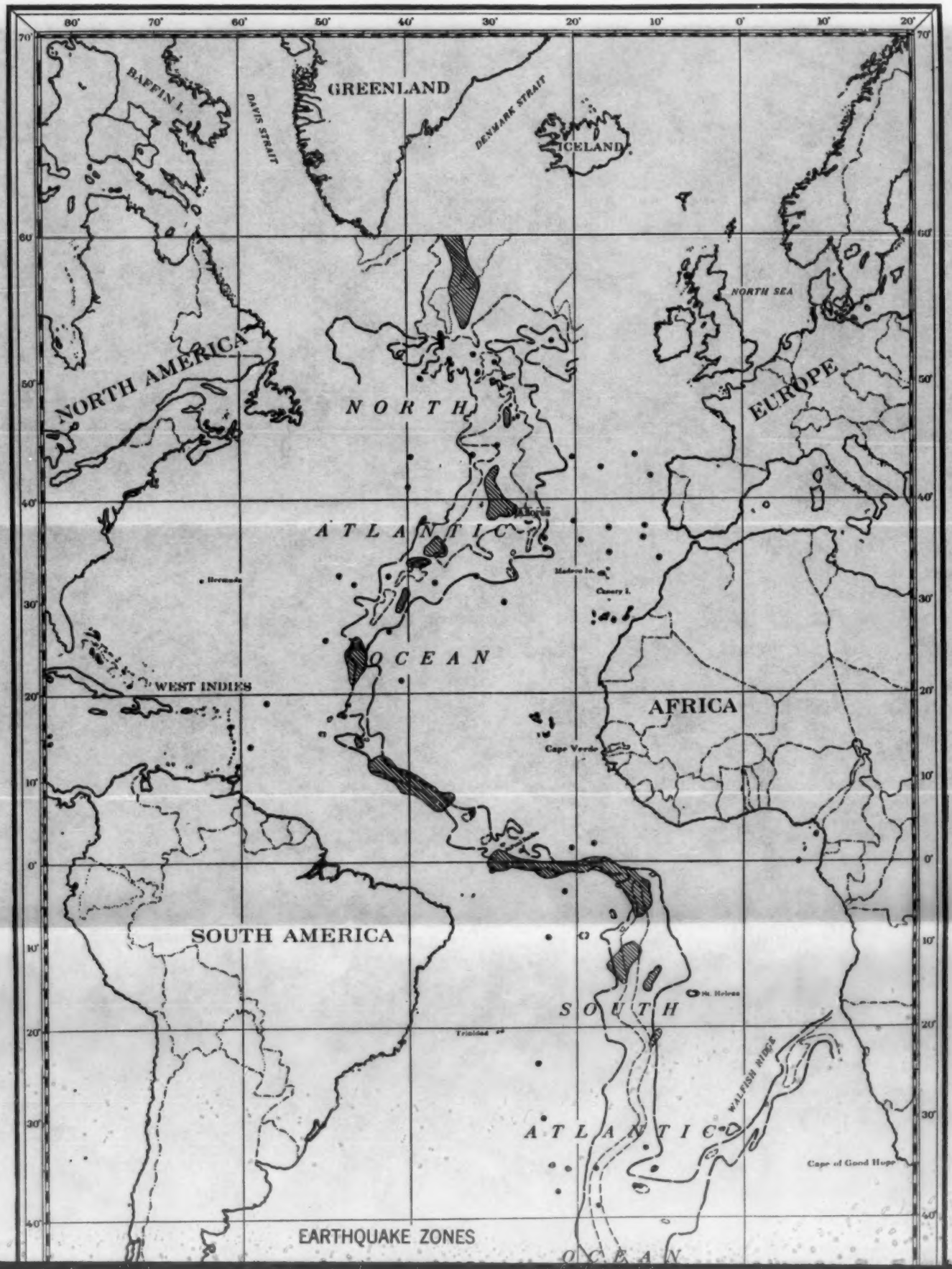
The first feature to be discussed is the Mid-Atlantic Ridge which writers have associated with the lost continent of Atlantis and which has also been considered a remnant left behind in the parting of the continents according to the

Wegener hypothesis of continental drift. The idea comes from the peculiar and indeed unique configuration of the Ridge which extends the entire length of the North and South Atlantic Oceans and which, throughout its length is approximately half way between the continents and roughly parallel to their shores.

The information regarding the Mid-Atlantic Ridge is quite inadequate and it is even rather difficult to outline its boundaries. The 12,000 foot or 2000 fathom curve has been selected (Fig. 1) and on this basis the Ridge has widths varying from less than 100 miles to nearly 700. The part of the Ridge which has deep water on either side from about 53° N. Latitude to 36° S. Latitude has an area of 2,750,000 square miles or about 11/12 that of the United States. The part of the Ridge which has less depth than 1500 fathoms is outlined, but the information is too inadequate to make it possible to show other curves of less depth. The Ridge in general rises from 3000 to 6000 feet above the ocean bottom on either side, though some of the island peaks rise as much as 18,000 to 20,000 feet above it. North of 53° N. latitude the 2000 fathom curve is discontinued because of the decreasing general depth of the ocean and the approximate 1500 and 1000 fathom curves are shown.

It is of interest that the first recognition of the existence of a ridge came in 1854 in the course of the surveys for the first transatlantic cables. The discovery of Faraday Hills with a depth of less than 700 fathoms gave rise to a hope which was not realized that a great plateau of moderate depth would be found. The actual depth over most of the Ridge is considerable.

The curves shown between 60° N. and 47° S. are taken from three bathometric charts of the International Hydrographic Bureau at Monaco all of which have been revised to within the past two years. The knowledge of the southern portion of the Ridge is to be credited largely to the work of the German surveying ship *Meteor* since the World War. However even the most casual inspection of the charts shows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OCEAN
WEST INDIES
AFRICA
Cape Verde
SOUTH AMERICA
St. Helena
SOUTH
Trinidad
WALFISH RIDGE
ATLANTIC
Cape of Good Hope
EARTHQUAKE ZONES
OCEAN

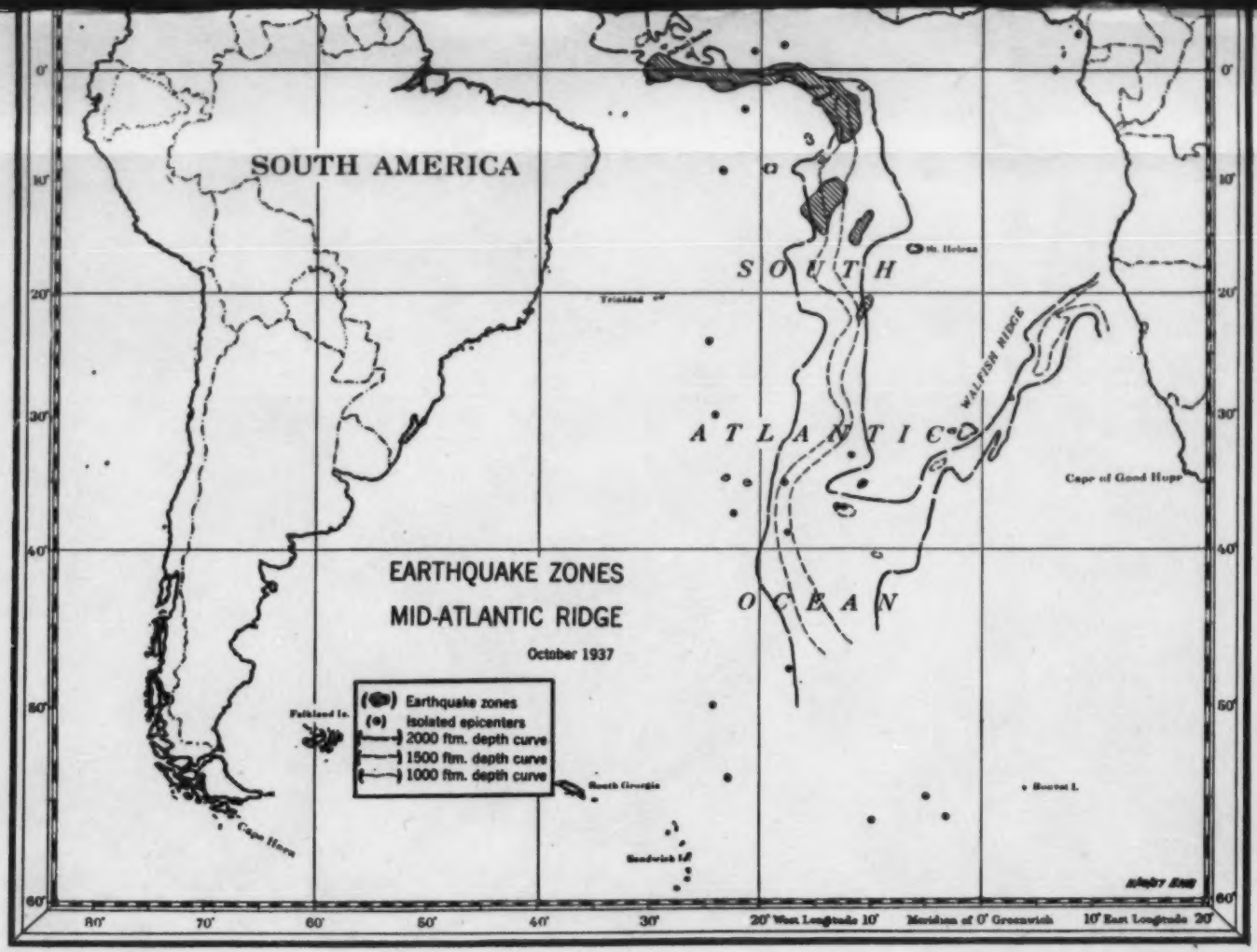
SOUTH AMERICA
S O U T H
A T L A N T I C
O C E A N
WALFISH RIDGE
St. Helena
Trinidad
Cape of Good Hope
Falkland Is.
South Georgia
Cape Horn
Sandwich Id.
Bouvet I.
EARTHQUAKE ZONES
MID-ATLANTIC RIDGE
October 1937
Earthquake zones
Isolated epicenters
2000 fm. depth curve
1500 fm. depth curve
1000 fm. depth curve
0°
10°
20°
30°
40°
50°
60°
80°
70°
60°
50°
40°
30°
20° West Longitude
10°
Meridian of 0° Greenwich
10° East Longitude
20°

FIG. 1.

the great need for additional soundings and makes it clear that nowhere have the surveys been intensive.

Figure 1 shows how closely the earthquakes are confined to the Ridge, how much of the Ridge has been seismically active in recent years, and how comparatively few are the earthquake epicenters in the basins on either side except between the Azores and the Iberian Peninsula. It is of interest, though beyond the scope of this paper, that the earthquake activity of the Ridge extends into the Arctic Regions, passing between the North Pole and Siberia and entering the continent to join the major earthquake belt in the vicinity of Kamchatka (Fig. 2).

The activity of the Mid-Atlantic belt was first worked out by Tams (1) for the period 1908–1926. There were many earlier reports from vessels experiencing earthquakes, especially in the equatorial portion, but since the uncertainty of the determinations may be on the order of 100 miles, these have not been included. Even in the case of instrumental determinations the recording stations are so distant that there is considerable uncertainty. The positions given by Tams for the period 1913–1926 in many cases differ materially from those given in the International Seismological Summary.

The earthquake areas outlined in figure 1 are made sufficiently large to take account of the uncertainty of position of the epicenters. In the absence of seismograph stations along the Ridge, only those earthquakes sufficiently strong to be recorded at distant stations are located and all others escape record. This is especially true of the southern portion. The long gaps between regions of seismic activity along the Ridge shown in figure 1 would probably be much shorter if all the earthquakes could be recorded, or if, even under present conditions, the record had extended over three hundred years, the approximate length of the oldest earthquake history of the United States. Imamura (2) considers even this period very short for the appraisal of the seismicity of a region.

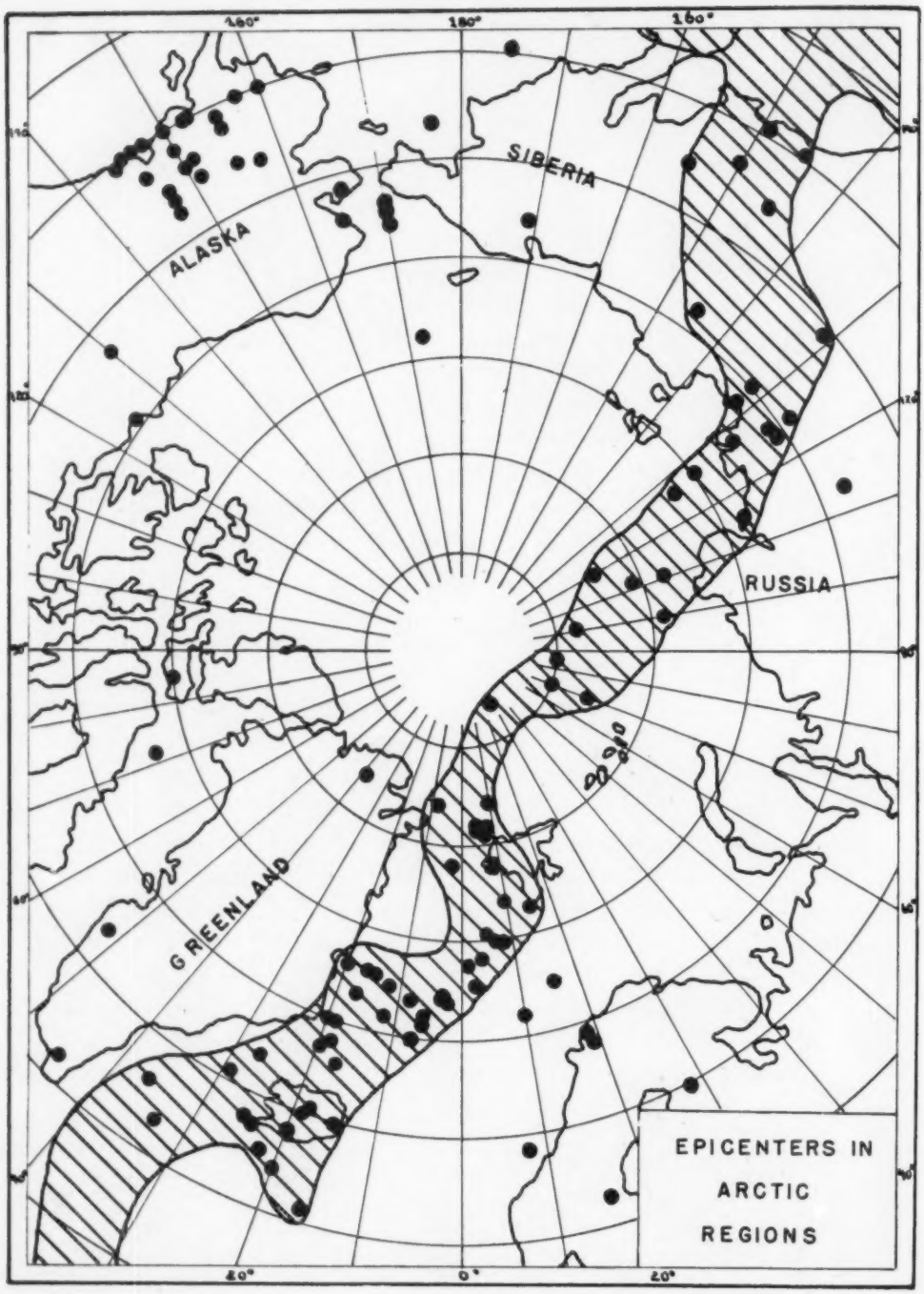
160°
180°
160°
SIBERIA
ALASKA
RUSSIA
GREENLAND
EPICENTERS IN
ARCTIC
REGIONS
40°
0°
40°

FIG. 2.

However there is sufficient evidence to show that while there are destructive earthquakes in the Azores and others of equal intensity at a number of places along the Ridge, the equatorial portion has the largest number of severe earthquakes.

The Ridge is without connection with the continents except for Walfish Ridge in the south. Likewise there is no connection with other earthquake belts except possibly to the east of the Azores. The western boundary of the Mediterranean portion of the major belt has not been defined and the epicenters shown on figure 1 may mean an actual connection between the two belts. However the charts indicate neither rises nor deeps though the bottom appears to be quite irregular. The distance between the Mid-Atlantic belt and the Caribbean portion of the major Circum-Pacific belt is so great as to preclude evidence of connection.

The delineation of bottom contours and the measurement of gravity at sea have been discussed. Further information regarding geologic conditions, including layers of discontinuity and to some extent character of the material, can be obtained by the application of seismic methods, using either natural or artificial earthquakes (explosions).

For a number of years the explosion method has been successfully used in shallow water, especially bayous, along the shores of the Gulf of Mexico, to ascertain the underlying geologic structure. In 1935 Dr. Ewing with others (3, 4) proved that it was possible to do explosion work out to the Atlantic Coastal Slope, which lies about 80 miles off-shore in the region of Chesapeake Bay. This work has made it possible to trace the crystallized basement (peneplain) rock beneath the Cretaceous and consequently to follow to seaward a fundamental continental structure whose type and date are known, (Fig. 3). A tribute is due to the Geological Society of America which supported the work and especially to the Woods Hole Oceanographic Institute which has made the work possible, as well as to many other coöperating institutions. Dr. Ewing has authorized me to state that he

has recently proved that apparatus developed by him can be used to a depth of 2600 fathoms, though there are still many problems to be solved, and he also considers it possible to explore the bottom materials to a depth of 10,000 feet. (He should be describing this work, except for the requirement that each participator in the symposium covers only one subject.)

The application of the explosion method is greatly needed in the study of the Mid-Atlantic Ridge as well as of the ocean basins.

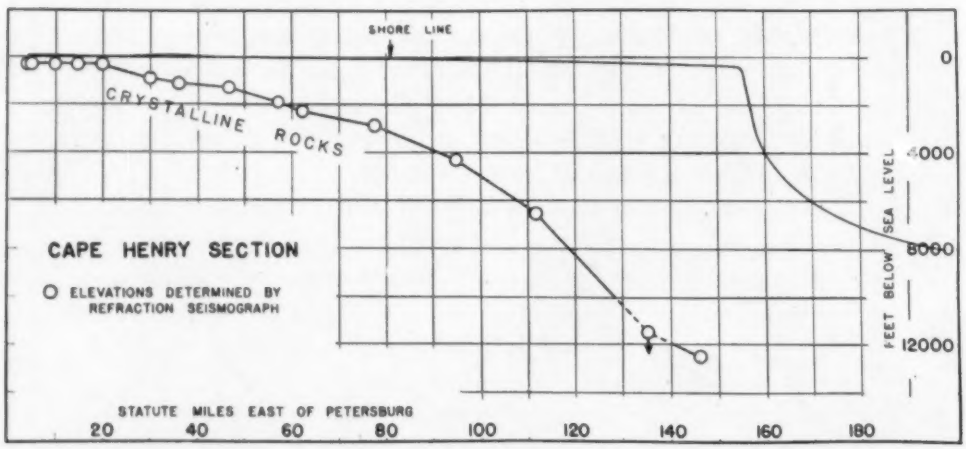

FIG. 3. Section showing the surface of crystalline rocks based on refraction seismograph methods by Ewing, Crary, and Rutherford in 1935. Modified from figure 32, p. 790, Vol. 48, *Bull. Geological Soc. of America.*

Explosions have the great advantage that time and place of occurrence are accurately known. However the waves sent out will give only very superficial information unless quantities of explosive, prohibitive in cost and dangerous to handle aboard ship, are used. For greater depths it is necessary to study earthquake waves since they penetrate to any desired depth. However for the study of the crust, earthquakes at a distance of only a few hundred miles are needed and special instruments are needed to record them. The great difficulty in the case of earthquakes is that the same waves which tell

about the condition of the medium through which they pass must also be used to locate the earthquakes including depth beneath the surface and to give the time of occurrence, and the two demands are not independent. This fact tends to limit the amount of information that can be obtained and its accuracy.

Ordinarily oceanic islands (those which are not merely the surface expression of a submerged continental area) since they are predominantly volcanic or coral, do not well serve the purpose of affording exposures of geologic formations which are one of the chief resources of the geologist on larger land areas. There are occasional exceptions and one of special interest has been pointed out by H. S. Washington (4)—St. Paul Rocks in the equatorial part of the Mid-Atlantic Ridge, which is composed of heavily metamorphosed peridotite or dunite, a plutonic rock usually found beneath the granite. From this Washington and others have deduced conclusions about the Ridge which should be investigated. The essential point regarding suboceanic geology is that the chief dependence must be placed on geophysical methods of attack.

The information regarding suboceanic structure from the study of waves from natural earthquakes is indicated by Gutenberg (5). He finds a layer of varying depth beneath the continents but on the order of 50–60 kilometers; a layer 20 kilometers thick beneath the Atlantic Indian, and southwest part of the Pacific Ocean; and a complete or nearly complete absence of similar layers in the rest of the Pacific and part of the Arctic Ocean. The picture is not one of essential difference in surface conditions but rather that the condition beneath the main Pacific is found all over the earth, and that over the Atlantic at a certain depth beneath the continents (Fig. 4).

The thickness of the continental layer varies from place to place and the same is probably true for the Atlantic layer. It is important that use be made of every earthquake along the Ridge to test this whenever possible.

So far as is known no deep focus earthquake (100 to 700 kilometers in depth) occur on the Mid-Atlantic Ridge. Since these are found chiefly along the rim of the Pacific Ocean in its northwest and southeast portion, the inference might be drawn that no unusual forces are at work in the Mid-Atlantic region.

From the seismic viewpoint four specific types of area can now be outlined where investigations are feasible by a properly financed expedition which are certain to produce results of major importance.

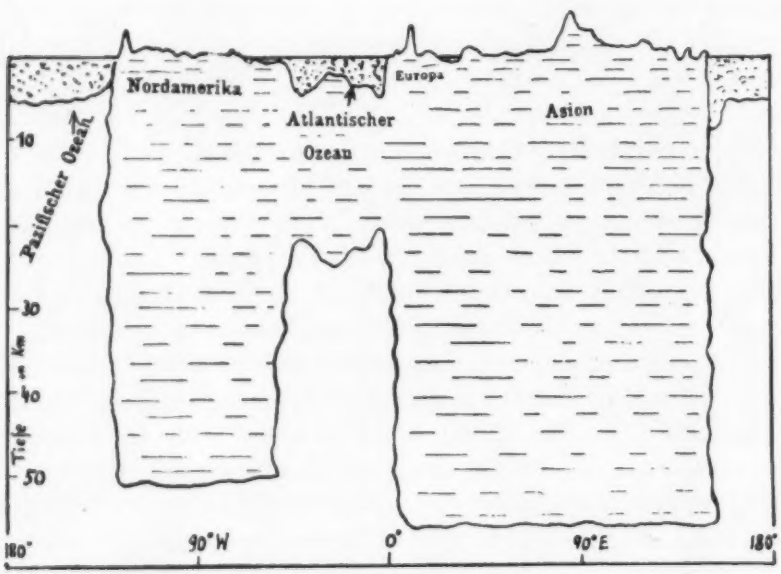

FIG. 4. After Gutenberg.

1. A ridge or rise area, preferably of the Mid-Atlantic Ridge.
2. An ocean trough or fore-deep.
3. An area of transition from continent to Atlantic Ocean with particular reference to depth of layers.
4. A similar area for the Pacific Ocean.

The complete examination of these areas includes: delimitation of bottom contours, with accurate determination of

position; determination of character of materials and layers of discontinuity by the use of the seismic method either with earthquakes or explosions; gravity determination; and bottom sampling with geologic study of cores. It may not be possible to carry on all of these and all cannot be done by the same expedition but the need for carrying on as many as possible and of coordinating what is done should be recognized.

1. Three areas of the Mid-Atlantic Ridge might be considered in the order of their desirability from the seismic viewpoint. The first is a portion of the equatorial section of the Ridge, preferably in the vicinity of St. Paul Rocks. This is a region of severe earthquakes and of great ranges in depth from ocean troughs to slight projection above the surface. The chief objection to this choice is the lack of a convenient base of operations for vessels doing sounding, gravity determination and artificial seismic work. The next choice is the vicinity of the Azores either to the east or west. In this region there are good bases for operations and it is possible to locate accurately all earthquakes. Also the depths are suitable for explosion work. Study of the area to the east might bring out important relations between the Mid-Atlantic and Mediterranean earthquake belts. The third choice is an area in the vicinity of Faraday Hills but this has as its only advantage nearness to Europe.

The location of earthquakes along the Ridge is important and does not have to await a special expedition. The requirements, suitable sites and means of continuous operation are hard to meet on oceanic islands. The situation in the northern part of the Ridge is going to be greatly improved by the project announced by Dr. Anghostino at the Edinburgh meeting in 1936 of the International Seismological Association. This is the installation of three sets of Bosch-Omori seismographs at three sites in the Azores, respectively in extreme west, middle and extreme east which will serve well for the location of all the stronger earthquakes. Previous efforts to locate the earthquakes from stations on the Ridge itself were not very successful because the instruments in-

stalled at Punta Delgada in the Azores, Ascension Island and St. Helena were of the Milne type, now considered obsolete. All published reports of earthquake epicenters have made no use of these stations. A recent station of great value in the northern part of the Ridge is that at Godhavn in Greenland.

The difficulties in the way of improving the situation along the Ridge in general are so great that the only recommendation ventured is for a seismograph at Bermuda which, though at a considerable distance from the Ridge, is well located geographically with regard to it, and which with a suitable instrument would add greatly to the accuracy of location. It fits in well with existing stations in Puerto Rico and Martinique. Some attempts are being made to develop a portable seismograph and future expeditions might instal these in their region of operations, during their stay.

There is still much to be learned about the exact relation of deep ocean troughs and the associated mountain structure whether continental or island arcs. Among other things it is important to have more exact contours of the boundaries of the troughs and this would make it possible to associate the earthquake epicenters with the movements which produce them. It would be very desirable to trace by means of explosions the basement rock from the shores out to the edge of the deep. With complete information many interesting relations develop as has been shown for the troughs in the East Indies and east of Japan. For some of the deeps there is apparently a rather uniform slope between the deep and the shores which does not seem to indicate typical mountain structure.

There are several deeps which might be studied. The most convenient to the United States are the Aleutian and the Nares, the latter following the Antilles on the Atlantic side from Santo Domingo almost to Trinidad. The Aleutian Deep, though better known than formerly would need much additional survey, the two seismic stations of the region at Fairbanks and at Sitka while well placed are rather far away and the region is subject to fogs and dangerous currents.

Accordingly the Nares Deep could well be chosen. While its present earthquake activity is small it might be found much greater with instruments for recording near earthquakes. Additional stations could be readily established Numerous soundings and gravity observations have already been obtained. The Bartlett Deep might also be selected.

Hess has called attention to the need for knowing the thickness of the layers beneath a Deep. Earthquakes rarely

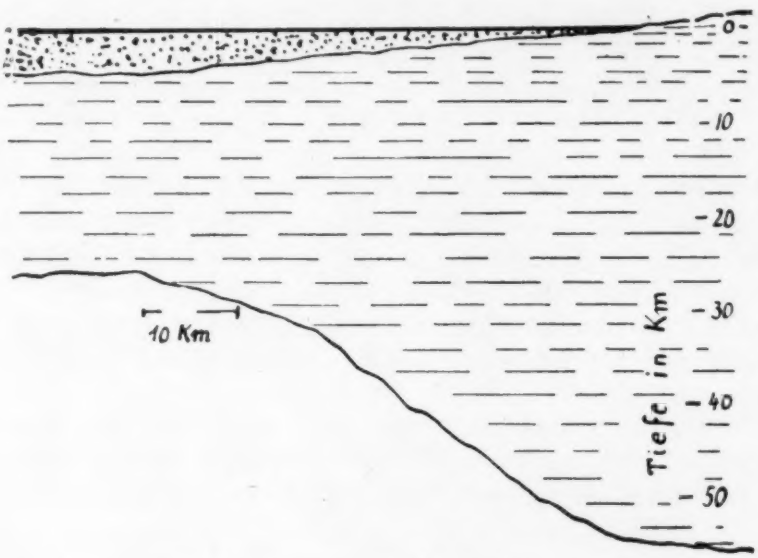

FIG. 5. Transition Europe to Atlantic Ocean. After Gutenberg.

occur in such positions as to give the desired information. It has been suggested that either by the use of two vessels or by the further development of the sono-radio buoy of the Coast and Geodetic Survey with a single vessel the problem might be worked out. A portion of the Nares Deep near its end near Trinidad might be used and waves might be sent through the earth with a moderate amount of explosive from one side to the other.

3. The method of transition of the various layers of discontinuity both shallow and deep from the continent to the

Atlantic should be further studied through earthquake and explosion. Gutenberg has inferred the manner of change from 50 kilometers to 20 as gradual west of Portugal, but other determinations should be made. This method of gradual transition is found also in the case of the basement rock by Ewing off the east coast of the United States (Figs. 3, 5).

4. In the Pacific Ocean there is evidence that the transition from a layer of perhaps 50 kilometers to zero is abrupt. This would indicate some kind of vertical discontinuity of considerable depth and this is exactly what Gutenberg has found. The transition of the basement rock from land to sea as determined by explosions is also of great interest.

In either case the line of attack is through a judicious combination of earthquake and explosion methods so that both the basement rock and the deeper layers are studied. Gutenberg has paved the way for the latter and Ewing for the former. Accordingly it would be well to make the studies for the Atlantic Ocean off the east coast of the United States and for the Pacific off the southern California coast where the available earthquake information is now very complete.

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TERRESTRIAL MAGNETISM AND OCEANIC STRUCTURE

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THAT irregularities of the distribution of the Earth's magnetic field over continental areas may bear definite relation to geologic features of the Earth's crust has been recognized for a long time. It is only in comparatively recent years that such magnetic data have been intensively considered as an aid in the determination of the character of geologic substructure. From accumulated observations it is possible to account for a considerable part of the Earth's field on the basis of a uniformly magnetized sphere. The difference between such a uniform or normal magnetization and that actually existing serves to bring out the general irregularities as a residual field (Fig. 1). Figures 2 and 3 present this feature in different ways. The Earth's outer crust or shell, perhaps 25 or more miles thick, is not homogeneous and therefore is not uniform in its magnetic behavior. Thus there are many regions of local magnetic disturbance, such as are caused by magnetic ore-deposits, some so great as to give rise to local poles and other irregularities of intensity twofold or even threefold the normal value in such regions. The anomalies at Kursk, Russia (Fig. 4), at Berggiesshübel, Germany, and in New South Wales are a few striking examples of such magnetic irregularities or anomalies. The great anomaly in South Africa (Fig. 5) apparently retained for over 100 million years by the huge Pilansberg system of Paleozoic volcanic dykes has been recently examined by H. Gelletich. Fourteen vertical dykes, of horizontal widths between 65 and 350 feet and horizontal extent up to 100 miles, are found to be strongly magnetized in a direction opposite to the present-

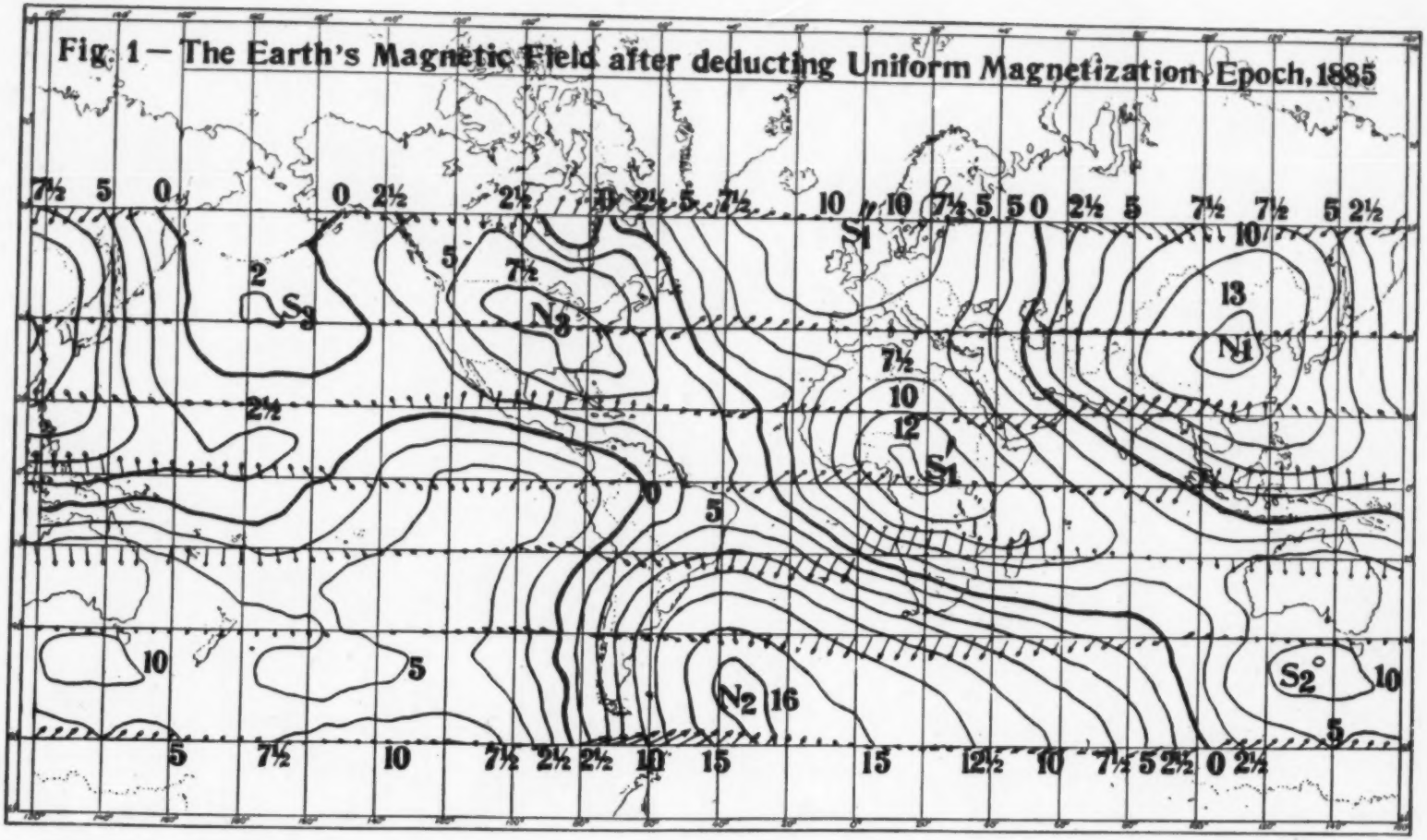

FIG. 1. The Earth's magnetic field after deducting uniform magnetization, epoch 1885 (after Bauer). [The curved lines are the lines of equi-residual vertical force. The arrows give the direction and relative intensity of the horizontal component of the residual magnetic force. Over the regions about the poles, N_1, N_2, and N_3, the north end of the magnet is attracted, while over the regions of the poles, S_1', S_1'', S_2, and S_3, the south end is attracted. The vertical intensities (large figures) are given in units of the second decimal, CGS.]

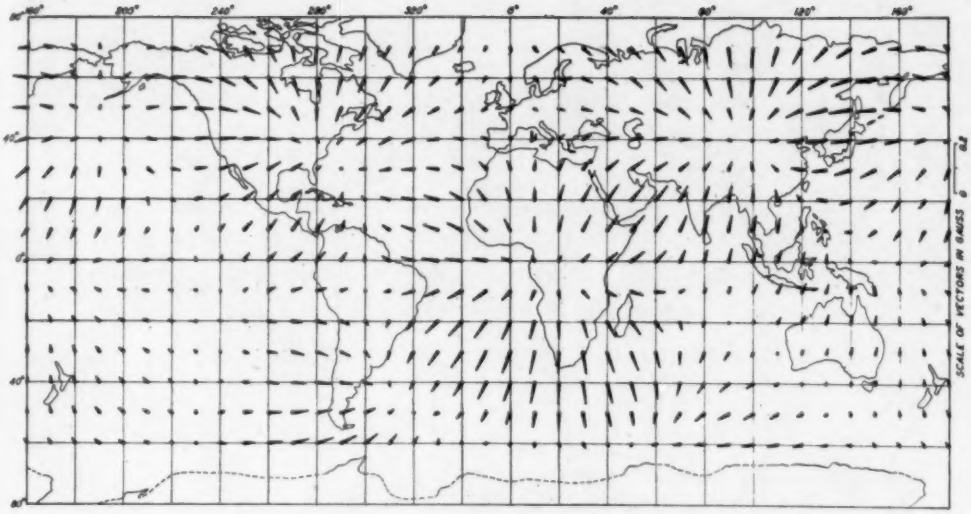

FIG. 2. Differences, observed minus eccentric-dipole field, or regional anomalies for horizontal vectors.

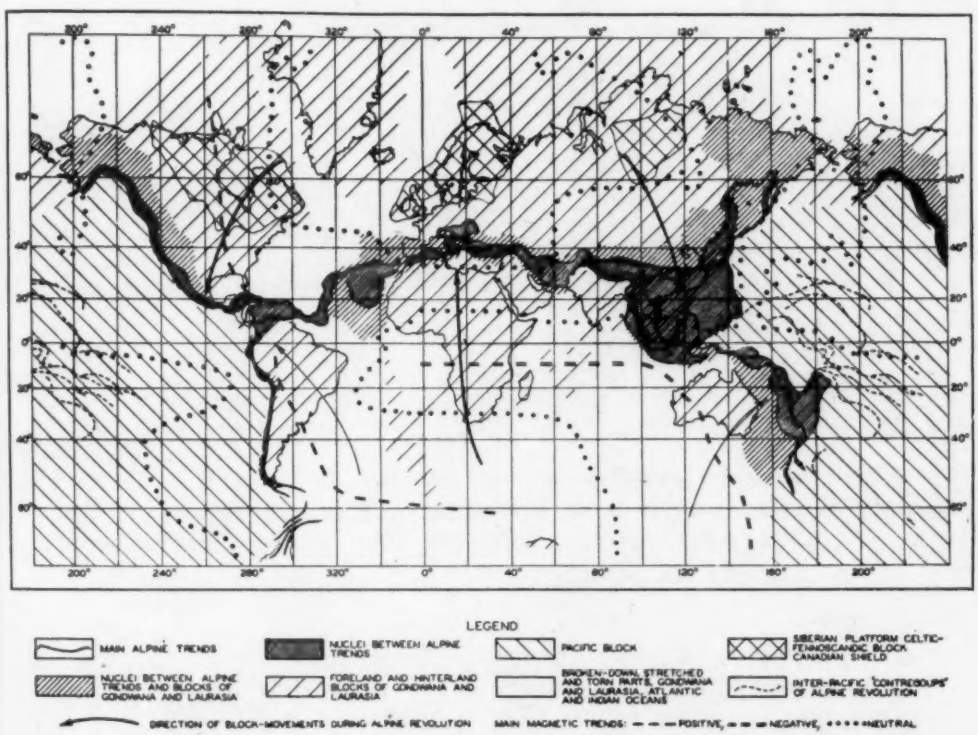
LEGEND
MAIN ALPINE TRENDS
NUCLEI BETWEEN ALPINE TRENDS
PACIFIC BLOCK
SIBERIAN PLATFORM CELTIC-FENNOSCANDIC BLOCK CANADIAN SHIELD
NUCLEI BETWEEN ALPINE TRENDS AND BLOCKS OF GONDWANA AND LAURASIA
FORELAND AND HINTERLAND BLOCKS OF GONDWANA AND LAURASIA
BROKEN-DOWN, STRETCHED AND TORN PARTS, GONDWANA AND LAURASIA, ATLANTIC AND INDIAN OCEANS
INTER-PACIFIC 'CONTRESOUPS' OF ALPINE REVOLUTION
DIRECTION OF BLOCK-MOVEMENTS DURING ALPINE REVOLUTION
MAIN MAGNETIC TRENDS: — — — POSITIVE, = = = NEGATIVE, • • • • NEUTRAL

FIG. 3. The main Alpine trends and magnetic trends of the Earth (after R. Staub and Jenny).

day magnetic field of the Earth. In this general region of South Africa the magnetic horizontal component of the Earth's field has decreased 16 per cent of its present value in a 30-year period!

The anomalous magnetic features thus observed over continental areas must have their counterparts in the great oceanic basins. We therefore may look confidently to such features as an aid in the study of oceanic structure. Indeed,

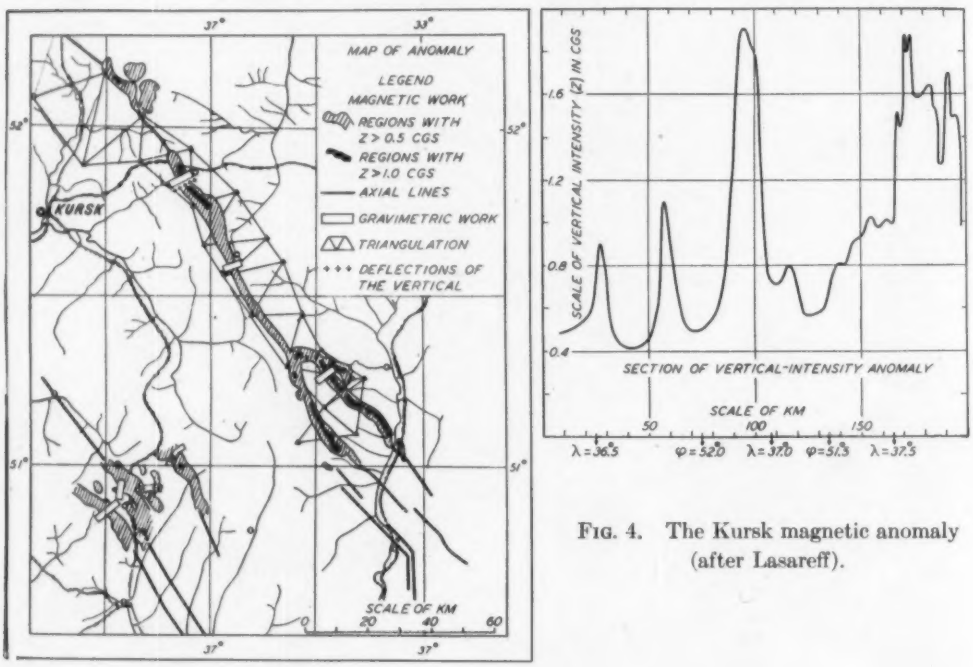

FIG. 4. The Kursk magnetic anomaly (after Lasareff).

observations made on isolated islands and island groups almost invariably indicate abnormal magnetic conditions, and the magnetic survey of the oceans by the Carnegie Institution of Washington since 1905 (Fig. 6) has revealed such conditions at sea.

However, the application of magnetic-survey results to the investigation of crustal features of the ocean bottom depends upon the existence of a difference in substructural

physical properties as well as upon the magnitude of this difference. Here enters the magnetic susceptibility of rocks, some of which are strongly magnetic, as iron-ores and igneous and metamorphic rocks, and some weakly magnetic, as most

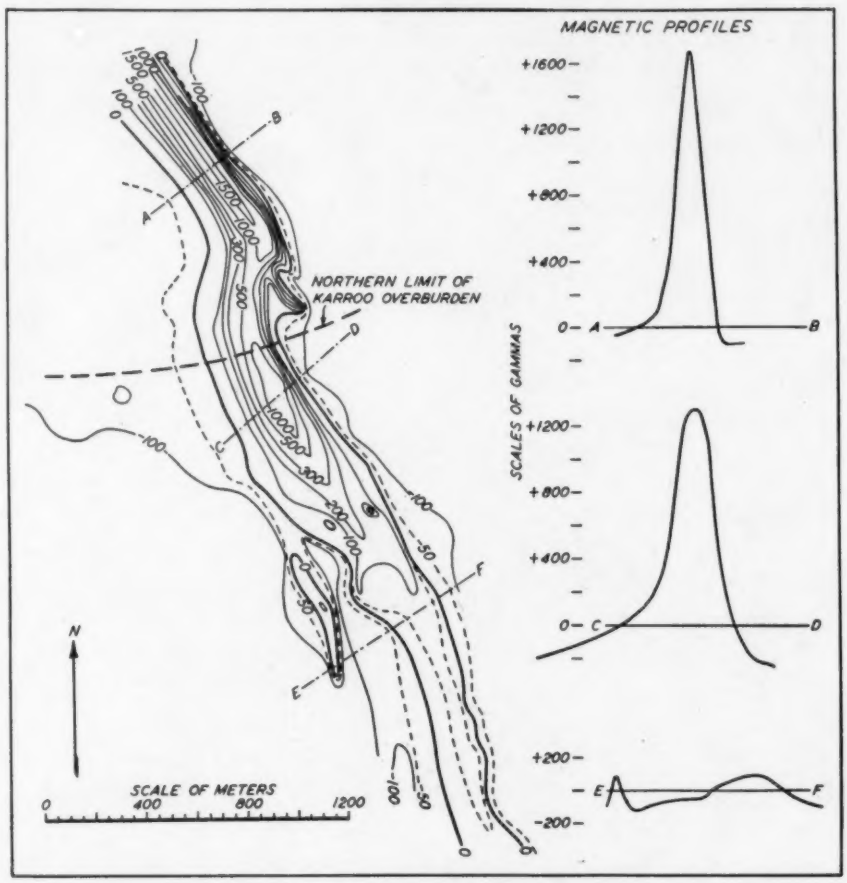

FIG. 5. Magnetic anomaly of Pilansberg Dyke System, southern Transvaal, showing anomaly of vertical intensity expressed in gammas (after Gelletich).

sedimentary rocks. Here, then, one must think of the observed magnetic anomalies as a kind of general reconnaissance method to indicate areas for other more intensive geophysical studies. However, the magnetic method of

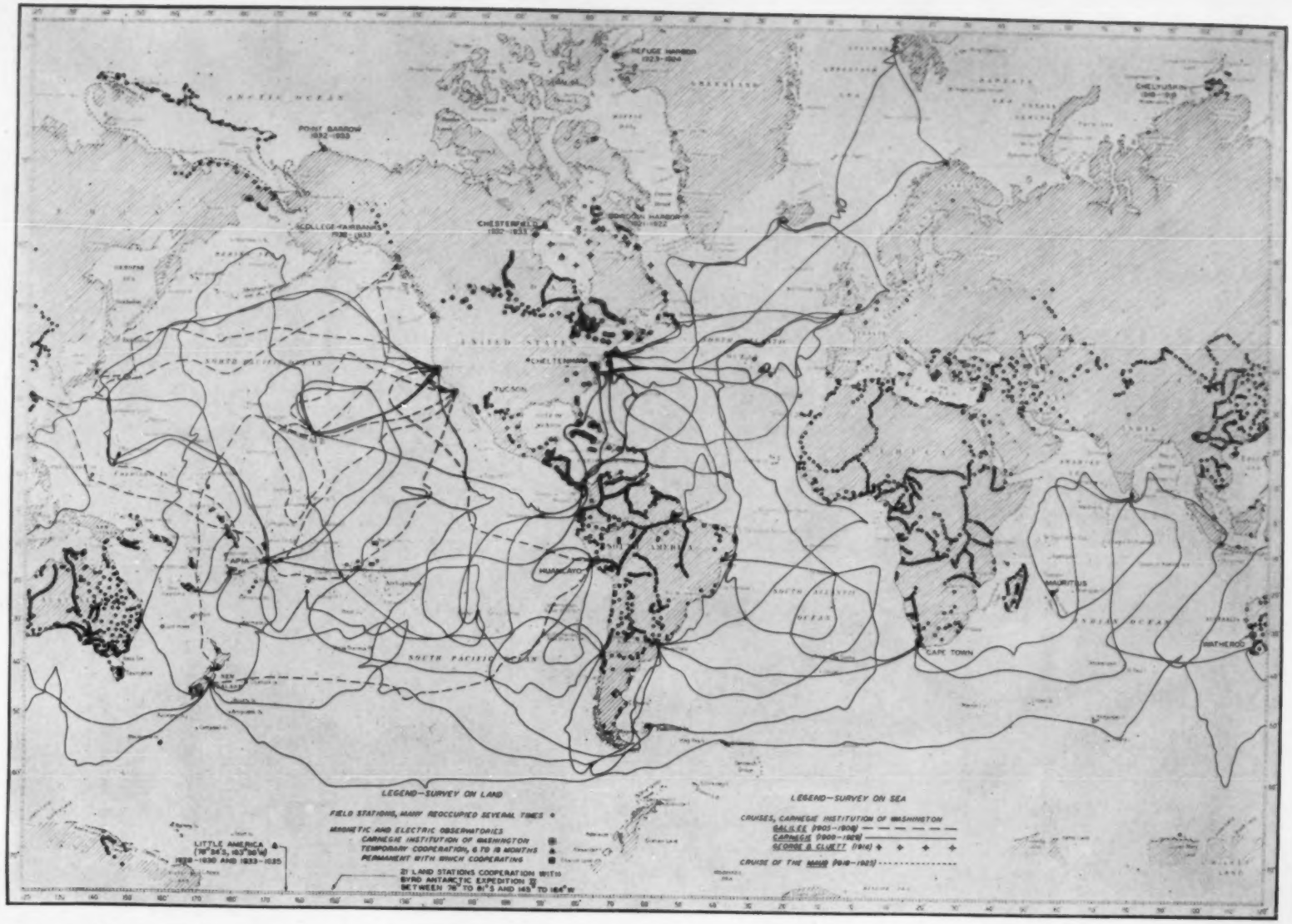

FIG. 6. World magnetic and electric survey, Department of Terrestrial Magnetism, Carnegie Institution of Washington, 1905–1937.

approach lacks depth-control, and therefore the size of geologic features must increase in proportion to depth if we are to detect such features. Thus, unless the magnetic anomaly observed at sea is over shallow water or if over deep water is of enormous proportions, there may not be great hope of immediately deriving very exact information regarding particular areas of bottom substructure from distribution data as shown only by isomagnetic maps.

The Earth's magnetic field is subject to changes with time, that is, secular variation—a phenomenon, adequate data for and investigation of which afford a field of large potential value in the study of the Earth's crust. Herein is the great need of continuance of the accurate magnetic survey of the oceans that we may determine the character of such changes with epoch and their accelerations at various places.

Ever since the discovery of the change in the Earth's magnetic field through the centuries, investigators have noted the apparent asymmetry and regularity of secular variation when attention is confined to a single station or to a small part of the Earth's surface such as western Europe—only one-hundredth of the Earth's surface. Considering, however, the actual observational data when the whole Earth is taken into account, secular variation is not so simple a phenomenon that it may be explained by periodic movements of the Earth's magnetic poles and the like, but is a regional phenomenon connected with a large geological structure—oceans and continents—of the deeper layers of the Earth's crust.

Secular-variation data are determined from isomagnetic charts for various epochs (Fig. 7). Changes in compass-direction due to secular variation are shown in Figure 8. In succeeding epochs, shifts of the isomagnetics occur. Lines or contours joining points of equal change are called isopors. The foci of rapid isoporic changes (Fig. 9) raise questions of broad geophysical significance. Isopors in vertical intensity are shown by Figure 10. A large part of these changes may have origin in influences impressed by forces

FIG. 6. World magnetic and electric survey, Department of Terrestrial Magnetism, Carnegie Institution of Washington, 1905–1937.

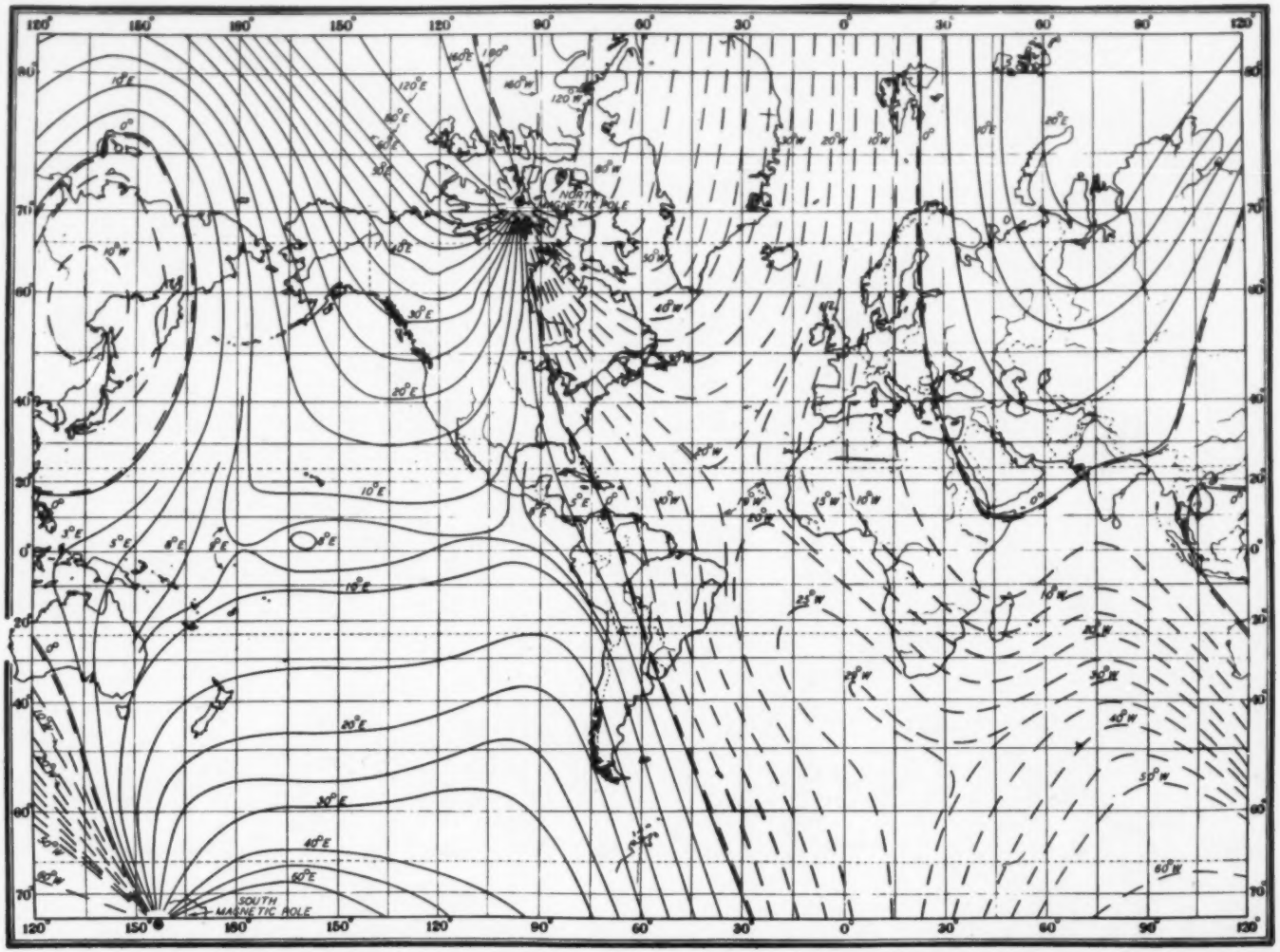

FIG. 7. World isomagnetic chart showing lines of equal magnetic declination, epoch 1935.

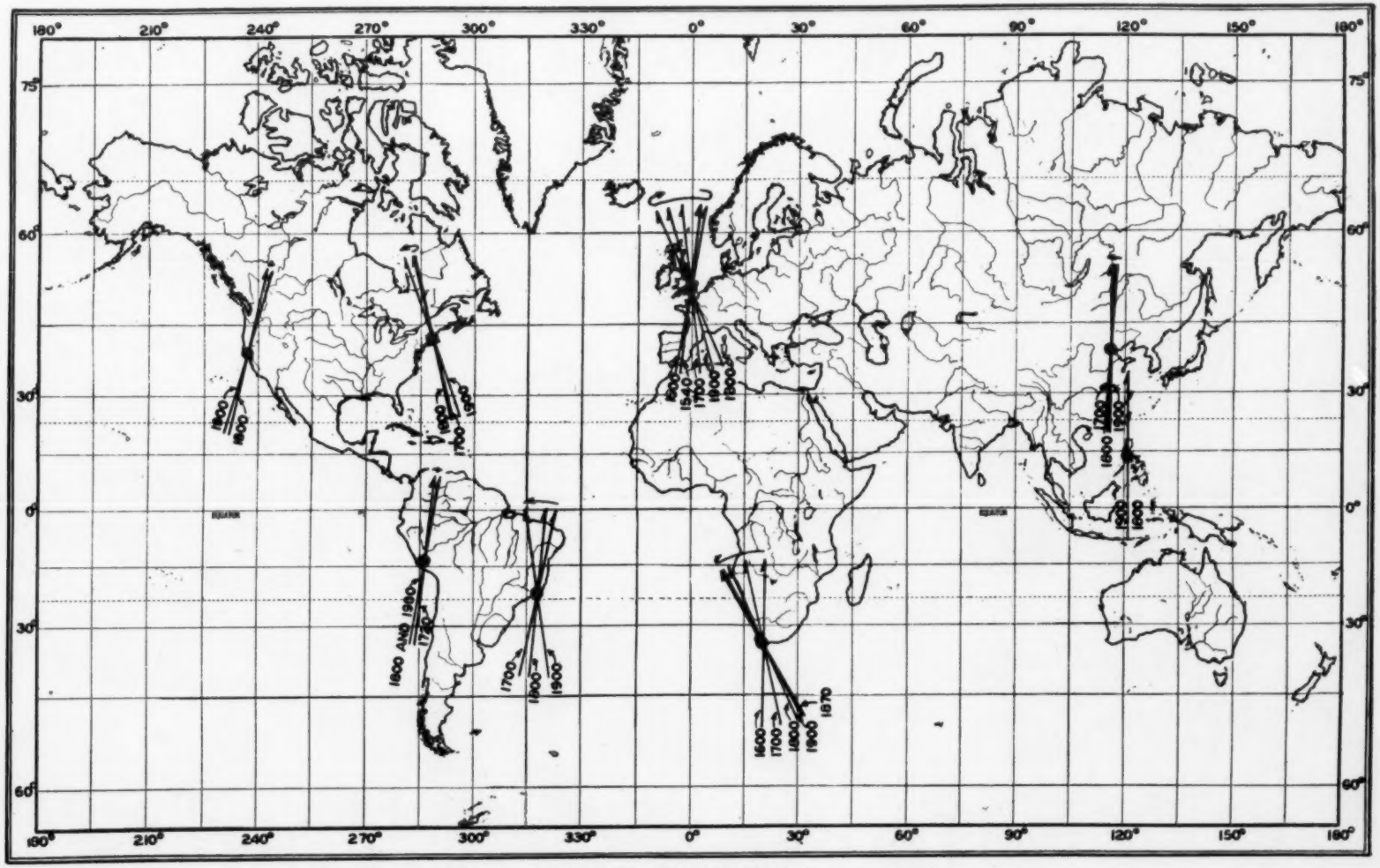

FIG. 8. Compass-direction at 1540, 1600, 1700, 1800, 1900, for various stations.

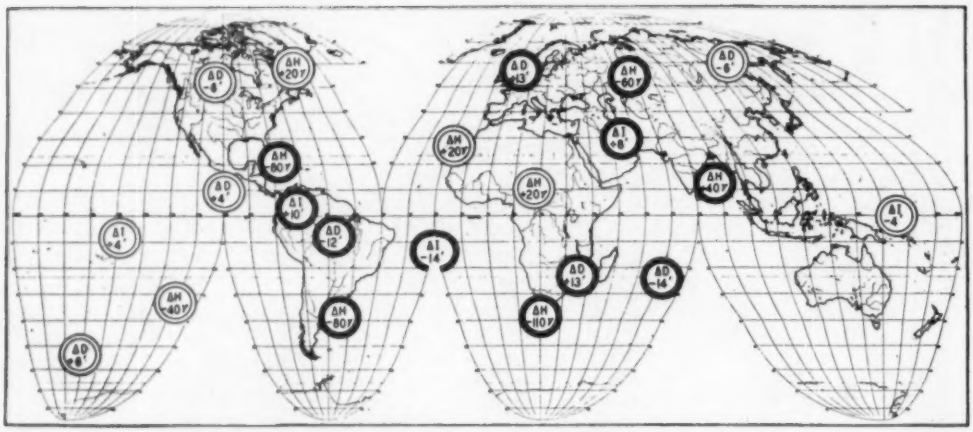

FIG. 9. Distribution of foci of rapid annual change of the magnetic declination, inclination, and horizontal intensity, approximate epoch 1920–1925 (after Fisk).

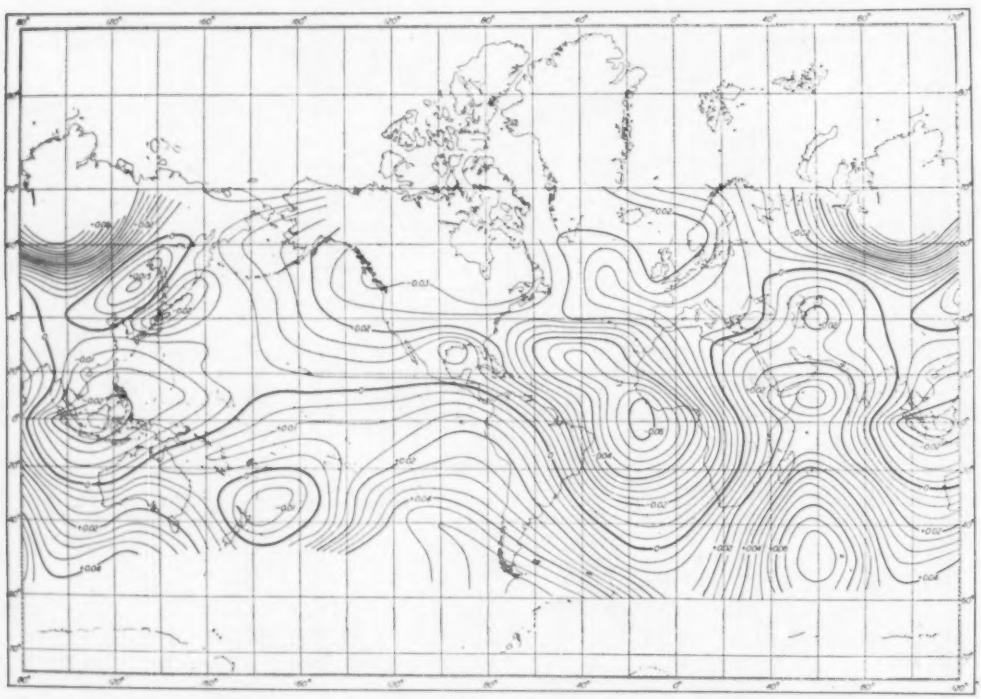

FIG. 10. Isopors total secular change in vertical intensity, 1885–1922 [isopor-interval 0.005 CGS unit; increase of downward magnetic force considered positive].

ceaselessly at work deep within the Earth. The rise and fall of the rate of secular change and the slow expansion and then the gradual retraction of the areas within which there have been excessive alterations in the magnetic elements are

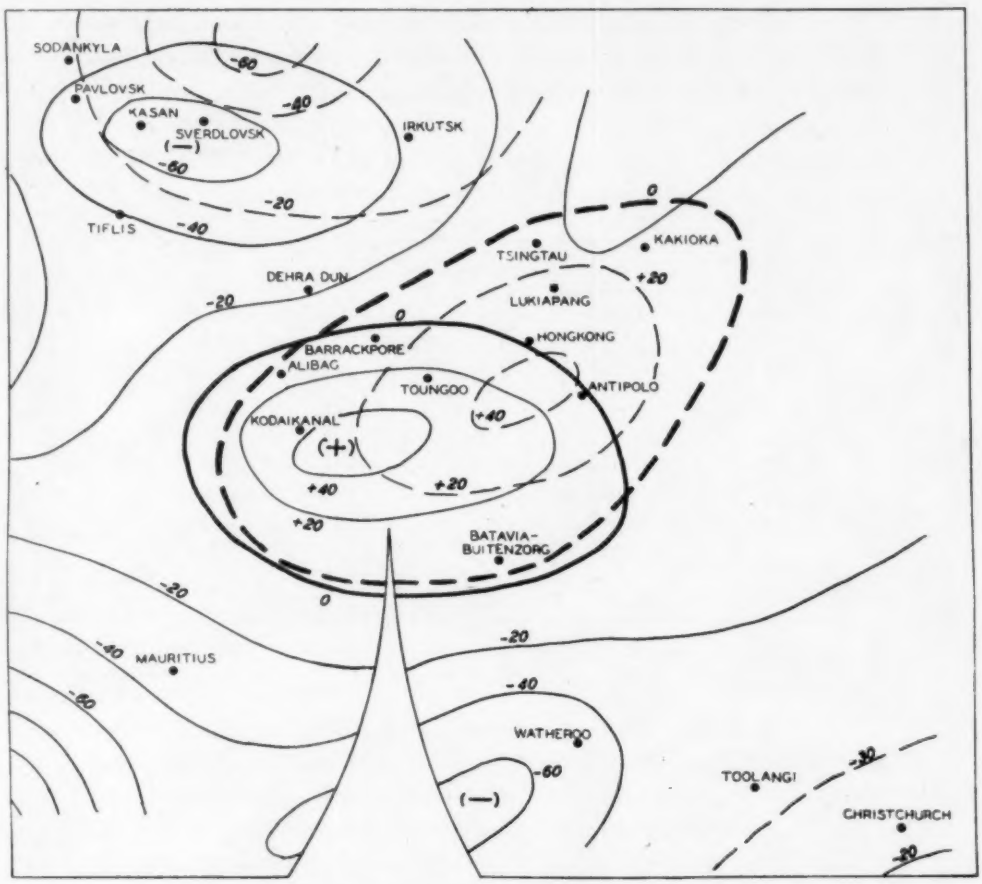

FIG. 11. Change in position of horizontal-intensity isopors, 1906–1918 [dashed lines for epoch 1906, solid lines for epoch 1918].

significant of such changes (Fig. 11). The distribution of isoporic foci is noteworthy. They are practically all in the hemisphere containing the great land-masses with the intervening Atlantic Ocean. Such foci as are found in the Pacific

Ocean are of but moderate intensity and not well defined. These relations to the surface structure of the Earth can be scarcely accidental; it is natural to suspect that there is a causal relation between crustal or subcrustal movements and conditions and these magnetic manifestations.

As an example, attention may be called to the apparent diminution of the intensity of the Earth's magnetic field which is marked over oceanic areas, especially in the Southern Hemisphere. The interpretation of such data doubtless will be important in geophysical and geological research to advance understanding and interpretation of Earth phenomena.

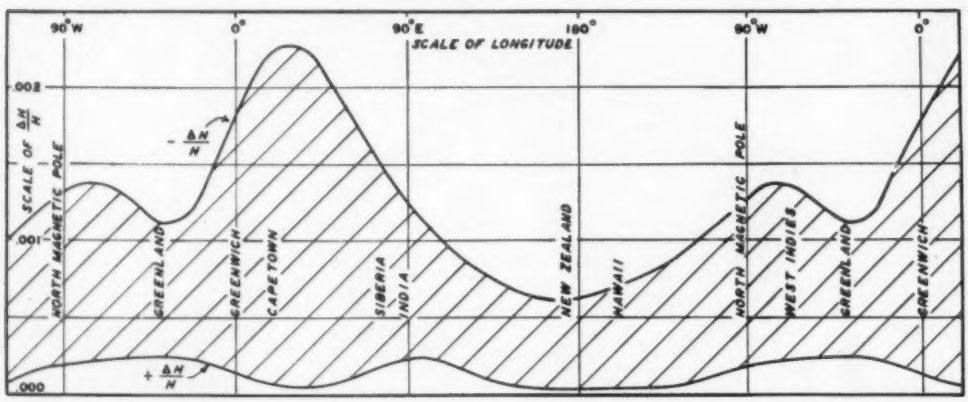

FIG. 12. Longitudinal distribution of proportion of annual change [$\Delta H/H$] of horizontal intensity (after Fisk).

For example, from observed earthquake-wave velocities and reflections for different regions and depths, Gutenberg estimates that the crustal layer, which under most of the continental and water-covered continental structures is about 25 miles thick, is either lacking or quite thin under the Pacific Ocean including possibly the arctic region. Under the Atlantic and Indian oceans this layer is of appreciable depth. Thus under the Pacific Ocean the basic surface of the Earth's mantle is practically exposed. There then we may expect different geological and geographical properties from those found elsewhere. Thus the observed longitudinal distribution of magnetic secular-variation (Fig. 12) agrees with that of

land-areas (after Fisk, based upon data from 75° north to 65° south *)—as witness the moderate rates of annual change over the Pacific as compared with those over the Atlantic and adjoining continental areas. A definite control is necessary for a number of epochs to facilitate the investigation of causes producing and governing these progressive changes which, it appears, would be favored by accurate knowledge of their accelerations and distribution. The importance of

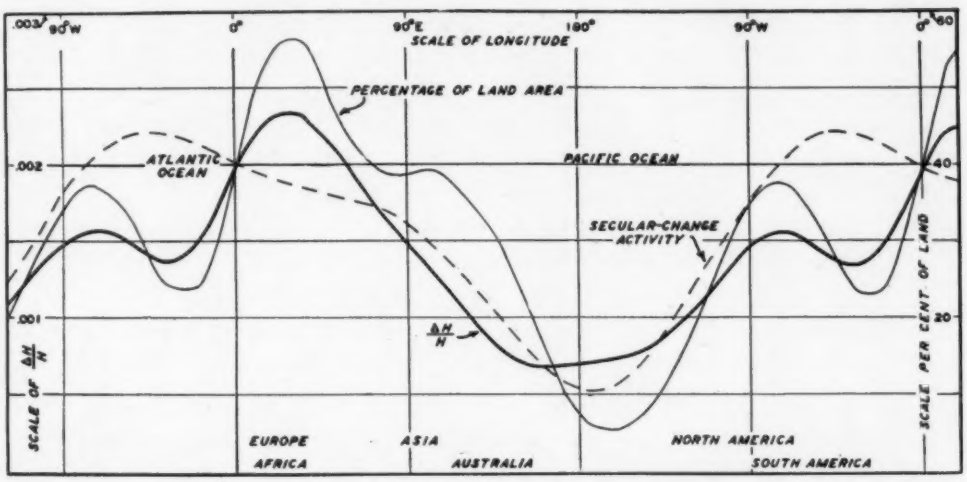

FIG. 13. Variation with longitude of $\Delta H/H$ [annual change averaged without regard to sign] of the distribution of the proportion of land and water areas and of secular-change activity approximately determined by the density of the distribution of isoporic lines (after Fisk).

the determination of secular-variation over the oceans may be readily seen by a study of Figure 13.

Further data bearing on correlation thus indicated between the surface-distribution of the secular-change activity promise conclusions concerning secular-variation processes localized in the crustal layer and only where this layer is present.

* In this graph showing distribution of annual change ($\Delta H/H$) of magnetic horizontal intensity the lower curve represents the average positive values of $\Delta H/H$ in each lune between the meridians, while the upper curve represents the numerical magnitude of the average negative values. Thus the shaded areas between the curves are measures of the excess of the negative over the positive annual change.

Thus continued secular-variation surveys at sea should bring together seismic and magnetic methods of approach to crustal adjustments and possibly gravimetric work.

There is an interesting possibility by which secular-variation information may be extended to times more remote than those for which observations exist. When lava cools and freezes following a volcanic outburst, it takes up a permanent magnetization dependent upon the orientation of the Earth's magnetic field at the time. This, because of small capacity for magnetization in the Earth's magnetic field after freezing, may remain practically constant. If this assumption be correct, the direction of the originally acquired permanent magnetization can be determined by laboratory tests provided every detail of the orientation of the mass tested is carefully noted and marked when it is removed.

Just as lavas cooling through the Curie-point in the presence of a magnetic field become magnetized in the direction of the field, so sediments containing microscopic magnetic particles also assume the direction of the prevailing magnetic field. This is in its truest sense "fossil" magnetization for the magnetic field of the epoch of deposition is embedded in the sediment as surely as the organic remains of that epoch. Whether this fossil magnetism remains as a connate property of the rock or whether it is destroyed by subsequent events even as organic fossils are sometimes destroyed, depends upon the postnatal experience of the sediment. In so far as it remains, it may supply a valuable record of the Earth's magnetism and an important clue to the history of the rock.

One of the projects outlined in the original program of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington was the measurement of the Earth's magnetism at great depths in the sea. This project may now be consummated by magnetic examination of the core-samples taken with Dr. Piggot's apparatus described in an earlier paper. In order that the fossil magnetization of the specimens may not be disturbed, a method of measurement has been developed at the Department permitting tests of

high sensitivity and accuracy without submitting the specimens to artificial magnetic fields. The cores are cut into one and one-half centimeter cubes; these are rotated in a coil containing many turns of wire (Fig. 14). The voltage generated in the coil is introduced into a tuned amplifier,

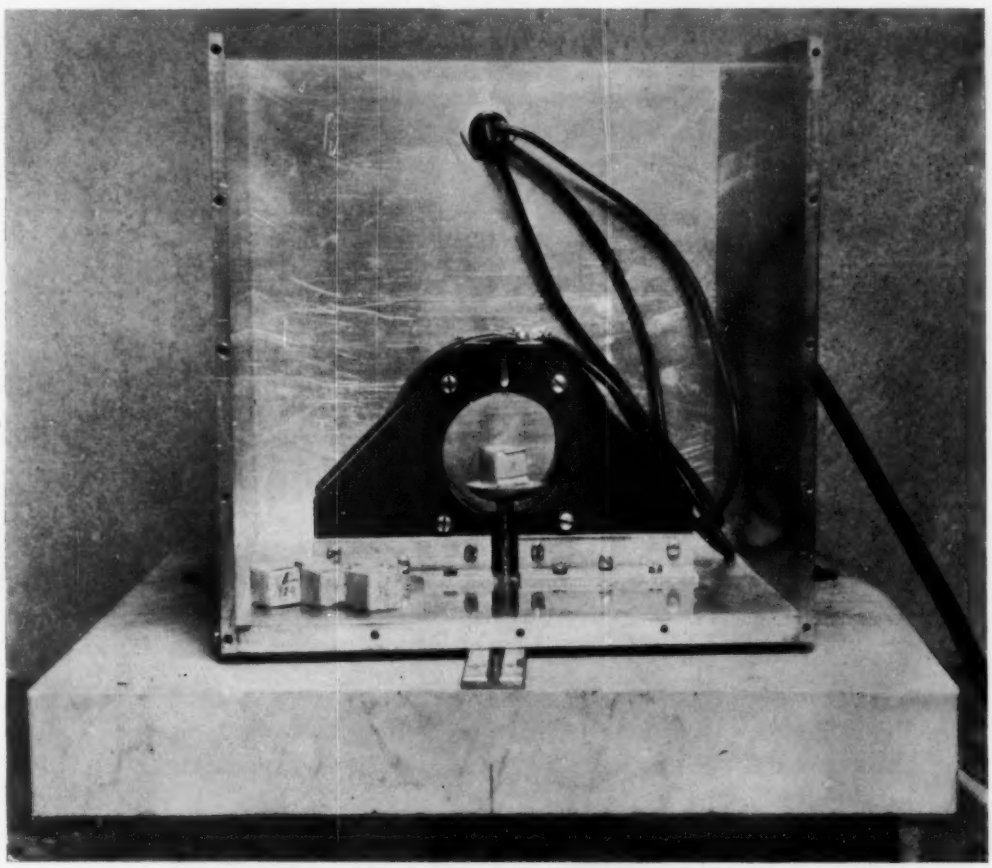

FIG. 14. Apparatus used for testing "fossil" magnetization.

and the alternating-current output of the amplifier is commuted and measured by a sensitive galvanometer. This equipment permits detection of the magnetic moment of a good steel magnet one millimeter long and one-hundredth of a millimeter in diameter. Through further refinements it is

expected the sensitivity of this method can be increased manyfold.

Tests just conducted on one core show distinct differences in the direction of magnetization at various depths. Decision as to whether or not these differences are the result of differences in the Earth's field at the time the sediments were deposited must await further study. If such is found to be the case, we have in the core-samples a means to extend the past history of the Earth's magnetism. Through interrelating individual cores by similarities in their magnetism, it may be possible ultimately to magnetically date the rocks.

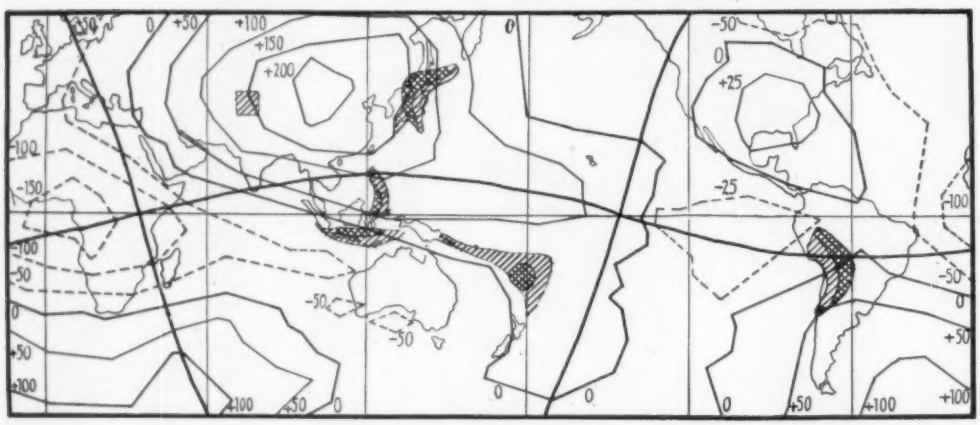

FIG. 15. Anomalies of vertical component [0.001 CGS] and principal deep-focus regions (after Visser).

This, too, would determine more accurately than surface observations the extent and character of the regional magnetic anomalies of the ocean bottom.

With improved knowledge of magnetic anomalies the question of interrelations with gravimetric anomalies and deep-focus earthquakes may be answered. It frequently is noted that gravimetric and magnetic anomalies occur in the same regions. Visser has recently noted an apparent relation that areas of deep-focus eathquakes are confined to the great regions of positive anomalies of magnetic vertical intensity around the Pacific Ocean (Fig. 15). Vening Meinesz has

found an excess of gravity in the oceans and a sudden decrease at the continental borders, and has demonstrated these deviations of isostasy cannot be situated in the Earth's thin crust but must extend far into the plastic layers below. There must result current-systems in the Earth's interior, downward below the oceans and upward below the continents. The chart of isoporic foci and their motions with time certainly lend strength to the idea indicating the interior of the Earth more mobile than the external layers, not only as a whole but regionally. Visser queries whether such interior currents may not convey material with special magnetic properties from the depth accounting for positive disturbances at the crust. May not then the anomalies of gravity and of terrestrial magnetism and the deep earthquakes have a common origin in current-systems in the inner Earth?

In conclusion, the facts of the geophysical observer of terrestrial magnetism in the field and in the laboratory may contribute much in the formulation of a complete picture of the Earth's crust and particularly of that great portion which is the ocean bottom.

ROUND TABLE DISCUSSION

PARTICIPANTS ADDITIONAL TO SYMPOSIUM SPEAKERS

ALFRED C. LANE, Chairman Committee on Age of the Earth, National Research Council, in the Chair

WILLIAM BOWIE, Chairman, Projects Committee, American Geodetic and Geophysical Union
ROY E. DICKERSON, Chief Geologist, Venezuelan Atlantic Refining Company
E. A. ECKHARDT, Geophysicist, Research Department, Gulf Oil Companies
C. L. GARNER, Chief, Division of Geodesy, U. S. Coast and Geodetic Survey
W. H. HOBBS, Department of Geology, University of Michigan
W. J. HUMPHREYS, Director (retired), U. S. Weather Bureau
CHESTER R. LONGWELL, Chairman, Division of Geology and Geography, National Research Council
Chairman, Projects Committee, Geological Society of America
H. N. RUSSELL, Director, Halstead Observatory, Princeton University
PAUL S. SMITH, U. S. Coast and Geodetic Survey
W. T. THOM, JR., Chairman, Committee on the Geophysical and Geological Study of the Continents, American Geodetic and Geophysical Union
T. W. VAUGHAN, Director (retired) Scripps Institution of Oceanography

ALFRED C. LANE, *Chairman:* Members of The American Philosophical Society and Guests: It has been suggested that we stand in memory of Admiral Patton.* (All rise and stand for a moment.)

MR. FIELD: May I say a word in connection with Professor Vaughan's book, "International Aspects of Oceanography," just published by the National Academy of Sciences, Washington, D. C., which appears to be a very comprehensive work, and which I believe some of us have not had the opportunity of going over as yet? It will soon be available, and I think that it ought to be called to the attention of this conference.

MR. VAUGHAN: I hoped my book would be distributed before this meeting. There are articles in it which deal with various topics presented here—a chapter by Vening Meinesz,

* Director Coast and Geodetic Survey, d. Nov. 25, 1937.

a chapter by Gutenberg, by Heck, and so on, and it ties in exactly with the discussion here.

MR. HOBBS: Mr. Chairman, I have been thinking a good deal of what Mr. Hess brought out, and later Mr. Heck, in connection with the island arcs—especially those off the Pacific Coast—and this had to do with the position of the great deeps, with the position of gravity anomalies, and their negative character particularly, but more especially of the great abnormality of gravity along these interesting arcs; and there was also something said about the presence of ultra-basic rocks under these same regions. It was said, I think by Mr. Hess, that the ultra-basic rocks in connection with these arcs belong to the first stage of the deformation. My own study of these arcs is exactly of the opposite character, namely, that the first effusions in connection with the formation of these arcs where the volcanoes appear on the inner or back side of the arc, are universally acidic or of moderate acid character, and that basaltic effusions are always further back toward the interior and belong to a later deformation, and I have produced a map covering a good part of the earth which shows this.

I have also taken up in my book, that is not at all well known, and may not have been in the minds of the speakers, the position of the deep in relation to the arcs. In the case of arcs of large radius and curvature, the great deeps uniformly appear on the convex side of the arc, whereas as I stated, a good many years ago, in the cases of many of the arcs with small radius of curvature, the deep is on the inside of the radius of the arc, contrary to the general rule which applies to the arc of large radius. I thought the statement of the other speaker was in contravention of that position.

MR. HESS: I tried to say if there were two or three deformations in the region, the *ultra-basic* rocks belonged to the first. I had no idea of correlating other intrusions with the tectonic action. West Indian deformations took place about the beginning of Upper Eocene and again in Miocene-Pliocene time. The ultra-basics go with the first deformation, but

not with the second. The ultra-basics are serpentinized peridotites.

MR. RUSSELL: Mr. Chairman, I hesitate to talk on some of these things, but the question of where the ocean water went when submarine canyons were formed interests me. According to the Hess-McClintock hypothesis the zone at which the ocean level did not change relative to the earth's surface was at approximately thirty-five degrees latitude—I don't know the minutes and seconds, because it depends on how large an area was land and how much was water within the torrid zone—but it has to be extensive land to change things much. How did the water get out of the ocean? It has occurred to me that it might be by the locking up of the water in the Arctic regions in the form of ice. Grant that precipitation is very low over the Arctic basin, and suppose that great glaciation occurs in several regions—say, to put it crudely, in almost a ring around the Arctic Circle—suppose there were huge masses of ice piling up there; wouldn't it flow northward as well as southward, and would not the glacier gradually fill up the Polar Basin, if there were time enough and ice enough, even if there was no heavy precipitation in the Central Polar Basin? Wouldn't that permit filling up the Arctic Basin chock-a-block to the bottom?

MR. HOBBS: Mr. Chairman, I do not think it is true that precipitation in the Polar Region is very low. I think it is quite the contrary, especially for the Antarctic Region. Consider the amount of moisture which goes away in the form of icebergs, and the amount of snow which is driven off in blizzards in the Antarctic Continent goes up into astronomic figures practically. I am not classing both Polar regions together. One is a very high plateau—a great dome of ice— and enormous masses of moisture go away in the form of snow and icebergs, some of which are ninety miles long. Greenland, of course, in the Arctic, is of similar character, but not so extensive.

MR. DICKERSON: Years ago in the Philippines we investigated the ocean bottom and had the rare opportunity of mak-

ing an investigation not only of the topography of the entire island deeps, but of the topography of the surface as well.

There is one test of the ice cap theory that has not been applied—the biologic test. Incidental to these studies, we had at that time in Manila a botanist and a zoölogist of eminence who had been there the space of twenty to twenty-five years, so from their data we were enabled to divide the Philippines into sub-provinces both faunally and florally.

The central islands of Panay, Masbate, and Negros are separated by water only about two hundred feet in depth. That constitutes a faunal and floral province. That is just about the requisite depth for land connections during interglacial times. The other provinces of the Philippines were exceedingly distinct from the central provinces of Panay, Masbate and Negros.

Another line of attack can be applied to the hypothesis of these vast withdrawals of sea water. Mediterranean oceans or mediterranean seas, properly speaking, occur not only in the type Mediterranean, but in the Philippines as well. These mediterranean seas would be vastly altered by a lowering of sea level by two thousand feet. Take the Mediterranean for example; if two thousand feet of water are withdrawn, the entrance of sea water into the Mediterranean would be stopped at once because at Gibraltar there is a depth of only three hundred twenty meters. There is an example of what would happen in the Black Sea which is separated from the Mediterranean Sea by a depth in the Dardanelles of fifty to seventy meters—just about Daly's estimate of the withdrawal of sea water in the glacial period.

By means of certain island floral and faunal provinces and by study of mediterranean seas it is possible to get a check upon the possibility of withdrawals of sea water of two and three thousand feet during the Pleistocene; I may say, in passing, the answer is quite firmly that this is not possible when viewed from these biologic viewpoints.

MR. LONGWELL: A suggestion made to the Geological Society recently seems to have considerable merit. It had

to do with the Atlantic ridge. If we can study some parts of that ridge—fairly shallow parts well removed from any islands—by means of the sonic depth finder and these other modern tools, and find there some submarine canyons, heading east and west, that would have a very strong bearing on this question of lowering the sea level. You could not very well call on Daly's density currents to explain such canyons in that position; but if the sea has been lowered several thousand feet it is very conceivable that such canyons have been cut. Of course, their absence might not mean anything one way or the other, but their presence would be very suggestive.

MR. FIELD: I might say, in reply to Mr. Longwell, that certain British geophysicists and geologists are interested in such an expedition, and that plans are being discussed with the British Navy for such an exploration of the Atlantic Ridge.

THE CHAIRMAN: Mr. Dickerson is an oil geologist and oil geologists have done a lot of work in gravimetry and seismology. Does he have anything to say?

MR. DICKERSON: Mr. Chairman, I merely want to ask concerning the possibility of using Dr. Fleming's device for direct measuring of cores from land holes. Could you get any variation in magnetic effect between the bottom of a six-thousand-foot hole and a core taken up about a thousand feet? In other words, is there any difference in the magnetic effect in the core in the Pliocene compared to a core in the Upper Cretaceous?

MR. FLEMING: Nothing has been done in that line yet, but I think we would very much like to do it.

MR. ECKHARDT: I have been engaged in geophysical work in the oil industry and I am impressed this afternoon with the differences in viewpoint. It is very much like this: It seems to me that this broad viewpoint of geophysics which we have been hearing about this afternoon is like being in a region where the trees are so far apart you cannot see any two at the same time; whereas in the case of applied geophysics, you are in the middle of a little spot where there are so many trees you cannot see out.

I think this symposium has been extremely valuable. It typifies an effort to get at some real data instead of the vast amount of speculation which has been indulged in in the past, and I believe that it should be encouraged. I think the Philosophical Society is to be congratulated in having fostered an opportunity of this kind.

MR. HECK: I would like to call attention to something Mr. Hess said about gravity anomalies. On the south side of the Caribbean is the only place I know where these marked anomalies do not parallel, at least approximately, earthquake belts. The earthquake belt extends from Trinidad southwest toward Southern Colombia, following the main Andes but the anomalies follow along the edge of the Caribbean.

MR. VAUGHAN: Mr. Heck has shown you these charts. A few years ago the International Hydrographic Bureau, although it had the facilities for publishing the charts, did not have the money for hiring draftsmen. The Philosophical Society made a very substantial contribution to pay a draftsman for putting together these charts.

MR. THOM: In discussions of the cutting of the suboceanic canyons and river valleys, disclosed by recent geodetic-geophysical surveys, it has been variously suggested that this cutting was made possible by the temporary lowering of sea level of as much as 12,000 feet. By some this lowering has been pictured as having occurred in certain latitudes only, whereas others have postulated that it was similarly effective over all oceanic areas.

Having had occasion to work in high-plateau areas, and having noted various features characteristic of such plateaus, I should like to ask any meteorologists present whether a regional lowering of sea level by ten or twelve thousand feet would not have caused profound climatic changes in the continental regions—changes so profound as to have left enduring and unmistakable evidence of their occurrence, both in the life-records and physical features of the land areas?

It seems to me that the assumption of any general lowering of such an extent can, for several reasons, be regarded as one

of very doubtful probability—even though the fact that sea level has fallen *relatively*, and at least locally, seems amply demonstrated by the submarine erosional features now known to exist. By what process the cutting of the suboceanic canyons was made possible we can guess,—but without additional limiting facts we shall perforce continue to guess, without appreciable scientific advance or advantage.

MR. CHAIRMAN: Suppose the sea level should drop twelve thousand feet—what would be the effect on the climate of North America?

MR. HUMPHREYS: It would be such as it now is on the very highest plateaus of Asia, more or less arid because of the fact that precipitation would occur chiefly at much lower levels. Not much water vapor could get up to that great height, but from it there would gradually accumulate, I fancy, a great deal of snow and ice. I think the Antarctic and Greenland answer the question.

MR. THOM: Mr. Chairman, as a structural geologist, I have been especially interested in this afternoon's symposium because of its demonstration that ways are now open for notable advances in my field of scientific interest. Students of structural geology have long been accumulating factual data bearing upon the three-dimensional form, space-pattern, and time relationships of the deformational crustal features of the continental areas—and great advances have been made in these studies—but each time workers in the structural field have sought to make an analysis of facts in hand such as should disclose the causes and *modus operandi* of crustal deformation, they have arrived at several conflicting and more or less equally plausible hypotheses, and have found that the needed decisive evidence was beyond reach—because it must be sought in the then-inaccessible suboceanic regions. Consequently, I feel that great importance attaches to the demonstration that the combined use of geological and geophysical-geodetic methods now makes it possible to carry continuous exploratory traverses from the continental highlands to the oceans' depths—thereby revealing, among other

things, the morphology of the sea floor; the true nature of continental margins; the presence or absence of "continental type" rocks in sub-oceanic areas; the form and extent of past and present geosynclines; and the possible existence of submerged portions of mountain systems—as, for instance, in the North Atlantic between the Appalachians and their apparent continuations in northwestern Europe.

I have no doubt that workers in many other fields are similarly elated that the developing geophysical exploratory techniques are opening such extensive and important new areas of investigation. And I feel confident that we shall, within a few years, make great progress in substituting fact for theory in many important scientific directions. In order to obtain the great amount of new data needed, a comprehensive program of coöperative work will obviously be called for—especially since the problems involved transcend the capacities of the agencies of any one country. However, that the necessary facilities can be obtained, seems reasonably certain, both because of the broad general interest of the problems under attack, and also because of their major scientific importance.

THE CHAIRMAN: I would like to ask questions of Dr. Piggot. These ten-foot cores would represent sediments of about two hundred and fifty thousand years.—It seems to me Dr. Schlundt, out in the Yellowstone Park, had a pretty good measure of age by the decay of radio-activity. Do you suppose you could apply that method to these ten-foot cores?

MR. PIGGOT: We were hoping we could. It is a difficult job because we cannot get as much radium as we would like out of the cores. There would be a chance to go through greater time intervals on top of the mid-Atlantic ridge. There may be other places where stratification has been so slow that cores would give you a longer record. I was able to make only one shot on the ridge, but if I had been able to control the ship at that place—or control the authorities on the ship—I would have stayed there till I got a ten-foot core at that spot by sending down more energy. I don't know

whether this material would give you the information you want, but it would represent a much longer period. If you lengthen the bit to fifteen feet and cut through more sediments ten or a hundred times slower in forming, which we hope to do, it would increase that age stretch considerably.

MR. FIELD: I would like to ask Dr. Piggot whether he thinks it is going to be possible to get down to great depths, and what will be the ultimate limit of length of cores that he will be able to get in oceanic depths?

MR. PIGGOT: We started with a five-foot core, moved to six, then ten; now we are trying to build apparatus that will get a fifteen-foot one. We are building a winch with 5400 fathoms of line on it, using tapered cable.

MR. HECK: I would like to mention a subject of which nothing has been said, namely, the very marked achievement of carrying explosion methods into these great depths. As far as I know, neither explosive technicians nor military authorities attempted this before this work. Mr. Piggot has gone to depths of 2500 fathoms, and has planned to go to 4000 or 5000. What is the pressure at that depth?

MR. PIGGOT: Five thousand fathoms—that is fifteen thousand pounds per square inch. Our greatest depth to date has been just a little under 3000 fathoms.

MR. HECK: I think we ought to recognize that this achievement is partly due to activities of the Naval service and such organizations as duPont's, and I understand their officials have been very coöperative in helping work out these problems.

MR. PIGGOT: I would like to add to that remark that I do not think my apparatus would have gotten anywhere at all if it had not been for the aid of Dr. Mackey, of the Burnside Laboratory of the duPont Company—their powder research laboratory—and Dr. Dew and Mr. Lloyd, who have worked on that. They were very enthusiastic in their coöperation and made absolutely fundamental suggestions.

MR. EWING: I would like to add to Mr. Piggot's remarks about the coöperation from the duPont people. When we

started out to make seismic measurements in 2600 fathoms during the past summer, I had not had any experience trying to fire high-explosive in such deep water. We had pressures in our work of about eight thousand pounds per square inch and our problem was very different from Dr. Piggot's. He wanted one kind of explosion—a sort of push; we wanted an explosion to disintegrate gun, apparatus and all, within as large a radius as possible, and frankly I doubt if I would have had one bomb go off, up-to-date, if I had not got some very excellent help from this same organization.

MR. SMITH: Mr. Dickerson's and Mr. Thom's remarks suggested that so far as is known we have not definitely found the seaward extremity of any of these canyons in any surveys the Coast and Geodetic Survey has made. We have traced some of them on the Atlantic coast to about 1650 fathoms depth, about 150 miles offshore. They are still evident, although the gradients decrease, and in one case on the Pacific coast, off Point Sur in California, there is pretty good evidence that what looks like the extension of the Upper Big Sur River on land continues under the sea for about 125 miles winding about a submarine mountain which stands 7000 feet from the floor of the ocean in depths of 2000 fathoms (12000 feet). That even stretches the limit of this postulated lowering of the sea beyond the wildest dream, if these submarine valleys are to be accounted for in that manner.

There is something else that Mr. Dickerson might be glad to know, and that is that the Cagayan River on the north coast of Luzon is one of the few that carries its submerged channel directly through the continental shelf. The Congo is another. A few others have been found to have their submarine channels running through the coastal shelf. Most of them have been obliterated at the coastal shelf. This applies particularly to the regions we have surveyed accurately by modern methods.

THE CHAIRMAN: I would like to ask Mr. Longwell or Mr. Smith of the U. S. Coast and Geodetic Survey whether some one had not asked the Project's Committee of the

Geological Society of America for a grant to explore one of these canyons.

MR. SMITH: I have been working with Dr. Veatch in contouring the charts from recent surveys off the Atlantic coast, and there are so many things coming out of the work I hesitate to comment on them at the present time except to say that there are a whole lot of facts that are most interesting, but before commenting we would like to give them more study.

MR. HOBBS: I wonder why it is necessary to give the discussion the peculiar turn it has taken? Why do we assume when we find certain fissures—sub-ærial fissures—beneath the sea, that there has been a lowering of the level of the sea, when it is so much easier to lift the bottom of the sea up, and it has been only the French who have taken this view to any extent? Why do we have to assume withdrawal of the water from the ocean when the other is a much simpler assumption? Nowadays we move continents—according to some authorities—as if they were ships at sea, yet we cannot accept the idea of vertical movements of the ocean bottom while the evidence is about us.

MR. DICKERSON: In reply to Mr. Smith, I wish to call attention to one notable Pleistocene river which has received very great study upon the part of the Dutch. The Molengraaf River rose in Sumatra, and formerly flowed north between Borneo and the Malay Peninsula for a distance of 1200 miles. I do not want to introduce at this time a fish story, but it is quite necessary. The fish fauna in this basin have been exceedingly well studied—the fresh water fish in particular. The fresh water fish fauna of South Sumatra, Borneo and Indo-China are essentially the same. There are something like forty or fifty per cent of the species in common to those now widely separated regions. The Dutch not only studied it from this point of view, but they have shown that in the great now-drowned plain between the Malay Peninsula and Borneo, a distance of about four hundred miles, you can anchor a ship anywhere in two hundred feet or less of water.

Not only that, but the South Palembang peneplain in South Sumatra slopes northward into the Molengraaf River and all the streams of the South Palembang peneplain of Pliocene age turned northward. Part of the tin mines—placer tin—are in one of these streams. In the Island of Banka the tin gravels are in one of these old Pleistocene tributaries of Molengraaf River. We may trace northward this river by means of a fish tale! Dr. Herry, now at Stanford University, made a study in the Philippines, on the Island of Palawan, and found that the residual fish fauna was Bornean, so that the Molengraaf River emptied somewhere in the China Sea off Manila.

MR. BOWIE: Mr. Chairman, this really has been a cheerful and helpful meeting. Some of us who have been working in the field of geophysics for many years have wondered at the slowness with which the geologists have employed mathematics, physics, chemistry and mechanics in attacks on some of their baffling problems.

It is very seldom that students in dynamics and structural geology agree. The reason for this would seem to be that they have not used enough geophysical technique to get the data that they should have in order to interpret what has really gone on in the earth, and what is likely to happen in the future. Since the geophysicists and geologists have developed instruments, methods, and techniques, it seems to me the time has come to bring into the picture all agencies that are interested in the use of data secured to solve economic problems.

I should like to ask this question: How are we going to do much in geodesy, geophysics, geology, and the interpretation of the data that may be collected, unless we have complete knowledge of the configuration of the earth's surface? There are only a few areas where there are good topographic maps (mostly in Europe), and those maps were made not for scientific or industrial purposes, but for war purposes.

In the United States not over twenty-five per cent of our three million of square miles of area have topographical

maps that are suitable for modern uses. Forty-seven per cent of our area has topographic maps, but half of them are of the reconnaissance type, and are of little value to the scientist, the engineer, the geologist, or others. We are spending large sums yearly on all kinds of conservation projects in the absence of adequate maps. No doubt much of that money is wasted.

If many of the naval vessels of the world could be assigned to a survey of the oceans, it would only require a few years' work to give us charts that would be invaluable in the attack on some of the problems that have been outlined here today. I think we ought to get some real facts on the topography of the ocean bottom and better maps of the land, then we would be able to make great advances in the fields of geology and geophysics.

MR. HUMPHREYS: May I sow just one or two seeds here? I tried them on the Geophysical Union but the soil was barren. I will try it here based on these promises that we are going to have help from the Navy and elsewhere. It is this: Why do we not know much more about the rate at which water that came on to the land in the form of ice and snow is now going off in the form of water? There certainly have been some changes in the amount of this ice in the past; we have had glaciation and deglaciation. They tell us we are still in the latter part of the Ice Age—and that the glaciers are retreating and have been retreating for a long while; nobody agrees as to the length of time involved.

What I want to know is what is happening now and what is going to happen a little later on. I have got so I am not interested in the past any more, but what is happening right now, and what will it lead to?

I think there is a way—certainly there would be if the earth would quit squirming—to find out to what extent it is going one way or the other. I am assuming now that the earth is not wiggle-waggling, going up and down, heaving and puffing and so on, but staying put. The place it is most likely to stay put is in the tropical regions not so far

from the Equator. On a number of islands, let us put down wells, connecting with the ocean with sufficient damping action so that the height of the water in the wells will not be affected by the tides, and equipped with suitable recording devices that will keep track of the level of that water year in and year out. If the glaciers are now melting at the rate they apparently have been going during the last ten or twenty thousand years, the change in sea level would be in the neighborhood of one-twelfth of an inch a year, provided the earth is not wiggle-waggling. I would like to see a number of such stations as these.

Of what use, do you say? If deglaciation continues at this rate, in a few thousand years the United States will be a dust bowl, if climatic conditions remain what they have been. Since storms necessarily follow in general around cold borders, when the ice came down over Europe to a considerable extent storms followed a more southerly course; the Mediterranean and North African regions were well watered. As the ice retreated toward the north, so too did the storm track, and the regions abandoned by storms became dryer. It would be much the same way here; as the ice would go further and further north the storms in general would be at higher and higher latitudes.

This proposal of wells is a perfectly simple one. They can be established on a number of suitable islands and by comparison with one another or by allowing each to keep its own record for ten, fifteen, or twenty-five years, we can observe the relation of this retreat of the ice to the rise of ocean level. This is based on the assumption that these islands are not going up and down.

MR. BOWIE: Of course, we have the tidal stations all over the world. I do not see why we cannot depend upon these. Take hourly readings year in and year out and the average cannot be more than a millimeter one way or the other from the actual mean sea level. Of course, if there are storms they cause abnormal tides, but in general the average of the hourly heights is close to the true mean sea level for a station.

At any rate, I do not see why Mr. Humphreys and Mr. Whitney should not get together on that in Washington and report here next year.

MR. HUMPHREYS: We have tried that and found it is difficult. The point is to have such damping that there is no response even to the lunar tides through the months, to have small tubes feeding into rather large basins which will not be appreciably affected in level by these tidal fluctuations; only by slow annual changes.

MR. BOWIE: Mr. Chairman, the Coast and Geodetic Survey found wells a hundred yards back from the sea near Atlantic City having tides in them corresponding very closely to the tides of the ocean, except that the range is not so great.

MR. VAUGHAN: Also, you could not get rid of ground water effects.

MR. HECK: In southeastern Alaska, there is a good deal of evidence of rise of coast line of three feet in twenty-five years, and it is still going on.

MR. BOWIE: The coast of southern Sweden is going up a foot or two a century. According to Nansen and others, that is simply coming back after the release of load by the melting ice.

MR. HUMPHREYS: Mr. Chairman, it is these blessed seismologists—I don't mean seismologists, but isostasists that confuse the problem. I take it if the earth would quit squirming, going down at one place in response to load, the thing could be measured, but if the earth is going to behave according to the way the isostasists want it to behave, of course, calculations are all out.

MR. BOWIE: Mr. Chairman, I want to tell you the isostatic and geodetic earth wants to stay still, but the meterorologists give us evaporation, erosion, sedimentation, unloading some areas and loading up others. The equilibrium is disturbed, and then we poor geodesists have to get it back again, and then the meteorologists come and upset it again. I do not know what we are going to do about it unless we eliminate the meteorologists.

MR. RUSSELL: Has anything been done yet to find out what the gravity anomalies are on the big deep opposite the west coast of South America along the Andes?

MR. BOWIE: We haven't done any work in that area, but really it would be most interesting. We have a few gravity stations over the deep to the west of Mexico, and I should like to make a comment about the gravity anomalies because some of you may get the idea the old earth is really in terrible shape on account of these little narrow belts of negative gravity anomalies. First, let me tell you that these anomalies are the outstanding unexplained differences between the theoretical and the observed values of gravity after applying all necessary corrections. The anomaly is a measure of the extent to which the isostatic conditions that are postulated do not exist. But these belts are very narrow—thirty or forty miles wide—or something of the sort—and usually alongside the negative anomalies there are positive ones. So the area as a whole—West Indies and East Indies as a whole—is practically in perfect isostatic equilibrium. How small a block would be independently in equilibrium I do not know. It may be fifty, one, or two hundred miles square. There is no such thing as these areas being ready to collapse, either popping up in the air thousands of feet or being depressed an equal amount. We have got some local phenomena in the regions of the East and West Indies, and Meinesz and Hess might be on the right track in postulating that weak material or material of low density is folded down to account for the great negative anomaly belt.

MR. FIELD: Mr. Chairman, I wonder if Mr. Ewing or Mr. Hess would care to say anything about the advisability of gravity determinations on the Andes, south of Panama?

MR. HESS: The negative anomaly belt in the Caribbean apparently enters the north coast of Colombia; probably it once followed the Cordilleran Central. It may well emerge again in the ocean south of Peru, thus the vicinity of the great deep off South America, in this region, is worth investigating.

MR. FIELD: Also, there is probably more sedimentation going on in the Peruvian coastal deep than in the East Indian and West Indian deeps.

THE CHAIRMAN: I wonder if the oil companies investigating the region around Lake Maracaibo could contribute to our knowledge on this subject.

MR. ECKHARDT: There is a great deal of oil work in Venezuela, but it has not been extended across the mountains. The eastern Venezuelan basin has been pretty well surveyed and those gravity data are available.

THE CHAIRMAN: But your negative belt is not on the mountain range proper?

MR. HESS: It is at sea north of the mouth of Lake Maracaibo. It might be picked up further east—just west of Trinidad, in Venezuela, if gravity work were done there.

MR. BOWIE: I would like to pay tribute to Mr. Eckhardt. His company has made gravity observations at probably five thousand or so stations in Louisiana, Texas, and other places, and he has furnished to the Coast and Geodetic Survey, data for some seventy or eighty of his stations. I do not know whether they have been reduced yet. They will be as soon as men are available for the work; then the data can be made public, and it will give us very closely spaced stations in that region—I think the stations were mostly in Texas.

MR. EWING: I hate to ask Mr. Eckhardt for many more favors since I have asked him to get me out of jams in connection with apparatus we used last summer, but I would like to ask where these gravity data are available for northern South America.

MR. ECKHARDT: We have them in our files. I do not mean they have been published, but it would be difficult to correlate our gravity figures with yours. We have an arbitrary starting point down there, and as far as I know there are no gravity stations within the entire area that would tie in with your marine gravity data. If you can make that correlation or induce Dr. Meinesz to do it, I am sure we would be very glad to supply any data we have.

MR. GARNER: While on the subject of gravity observations I thought the meeting might be interested in Dr. Lopez, of Venezuela, who requested the coöperation of the Coast and Geodetic Survey on the gravity observations down there this winter. It is entirely out of the question to attempt it now and entirely too early to predict what can be done in the future, but an expedition is being planned for next winter.

THE CHAIRMAN: Are there any further remarks? If not, the discussion is closed.

85°
80°
75°
25°
20°
15°
10°
FLORIDA KEYS
CAY SAL BANK
BAHAMA BANK
CUBA
I. OF PINES
+80
THE CAYMANS
MISTERIOSA BANK
+80
BARTLETT TROUGH
JAMAICA
SWAN I.
PEDRO BANK
ROSALINE BANK
+50+
PANAMA
COLOMBIA
SO
85°
80°
75°

75° 70° 65°

NAVY-GEOPHYSICAL UNION
GRAVITY EXPEDITION
1936-7

HISPANIOLA

PUERTO RICO

VIRGIN I. BANK

BROWNSON TR

BEATA RIDGE

TANNER BASIN

ARUBA

CURAÇAO

BONAIRE

AVES

LEEWARD TROUGH

LOS ROQUES

ORCHILLA

TORTUGA

VENEZUELA

COLOMBIA

SOUTH AMERICA

75° 70° 65°

PROVISIONAL GRAVITY ANOMALY CHART

H. H. HESS

GREATER THAN -150

-100 TO -150

- 50 TO -100

0 TO - 50

POSITIVE ANOMALY CONTOURS

(BOWIE FORMULA)

60°

55°

25°

20°

15°

10°

ON

ROUGH

ANEGADA P.

ANGUILLA

ST. MARTIN

ST. CROIX

SABA

BARBUDA

ST. CHRIST.

NEVIS

ANTIGUA

MONTSERRAT

GUADELOUPE

AVES

DOMINICA

MARTINIQUE

ST. LUCIA

AVES SWELL

GRENADA TROUGH

ST. VINCENT

BARBADOS

TOBAGO TROUGH

GRENADA

BLANQUILLA

LOS HERMANOS

TESTIGOS

TOBAGO

MARGARITA

TRINIDAD

+35

+24

H.O.Misc. No. 8890

85°
80°
75°
25°
20°
15°
10°
FLORIDA KEYS
CAY SAL BANK
BAHAMA BANK
CUBA
I. OF PINES
THE CAYMANS
MISTERIOSA BANK
BARTLETT TROUGH
JAMAICA
SWAN I.
PEDRO BANK
ROSALINE BANK
PANAMA
COLOMBIA
SO

75° 70° 65°

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TORTUGA

VENEZUELA

SOUTH AMERICA

COLOMBIA

75° 70° 65°

CARIBBEAN TECTONIC MAP

H. H. HESS

AXIS NEGATIVE ANOMALY STRIP

PROBABLE FORMER EXTENSION OF ABOVE

FOLD TRENDS, J=Jurassic, E=Eocene, M=Miocene or younger

DIRECTION OF OVER TURNING & THRUSTING near negative strip axis

FAULTS

POSITIVE AXIS INDICATED BY ANOMALIES

NEGATIVE AXIS INDICATED BY ANOMALIES

AXIS ACTIVE VOLCANIC ARC

AXIS AVES SWELL, possibly volcanic arc

60° 55° 25° 20° 15° 10°

TROUGH

ANEGADA P.

BOMB

ANGUILLA

ST. MARTIN

ST. CROIX

SABA

BARBUDA

ST. CHRIST.

NEVIS

ANTIGUA

MONTSERRAT

GUADELOUPE

AVES

DOMINICA

MARTINIQUE

CROSS

WARP

AXIS

AVES SWELL

GRENADA TROUGH

ST. LUCIA

ST. VINCENT

GRENADA

TOBAGO TROUGH

BARBADOS RIDGE

BARBADOS

BLANQUILLA

LOS HERMANOS

TESTIGOS

TOBAGO

MARGARITA

TRINIDAD

M+E?

3035

2890

3200

H.O.Misc. No. 8890

MODERN DYNAMICAL OCEANOGRAPHY: AN ACHIEVEMENT OF APPLICATIONS TO OCEAN OBSERVATIONS OF PRINCIPLES OF MECHANICS AND HEAT

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(Communicated by T. Wayland Vaughan)

ABSTRACT

This paper begins with a general historical review of researches on oceanic circulation conducted since the introduction, in the early part of the nineteenth century, of Lt. Maury's international coöperative plan of making and reporting ocean observations. The treatment throughout is non-mathematical and an attempt is made to furnish the reader who is not a specialist in the field with a comprehensive survey of progress already made and of problems to be solved. Both theoretical methods of calculating ocean currents from observations of wind, temperature and salinity, and observational methods of determining ocean currents are considered. Special attention is given to errors made in earlier attempts to apply to the ocean theories based upon laboratory experiments. Revisions of these earlier methods and the rapid developments leading to the more satisfactory modern ones are briefly outlined.

Then, following an introductory classification of the various forces that generate and maintain oceanic circulation there are general outlines of various methods of applying the principles of physical hydrodynamics to problems of oceanic circulation. There is also an elaboration of the theory of eddy-viscosity and turbulence which processes account for rapid diffusion of momentum, heat and chemical properties. Next a method is explained of calculating the circulation that would account for the departure of actual surface temperatures from their normal values.

The explanations mentioned above are restricted to the fundamental part of the mathematical-physical treatment of oceanographic problems involving only the simplest conditions and essential methods. The paper concludes with a supplementary discussion of revisions made necessary by a more detailed consideration of actual conditions such as nearness to coastal boundaries and irregularities in depth.

INTRODUCTORY STATEMENT

IN line with the trend in science an industry toward coöperation between specialists for the purpose of offsetting the limitations inherent in specialization, the preparation of a series of bulletins dealing with geophysics was proposed by the Division of Physical Sciences of the Na-

[1] Contribution No. 11 from the Scripps Institution of Oceanography, La Jolla, California.

tional Research Council. As a result of the efforts of a large committee of investigators appointed to carry out this undertaking, 9 volumes have been published. This paper is a brief general statement based upon the chapter on Modern Dynamical Oceanography written by McEwen for Physics of the Earth—V, Oceanography (Bulletin No. 85).

General Historical Review

Lt. Mathew Fontaine Maury of the U. S. Navy initiated and developed an international coöperative arrangement for making observations at sea and reporting them on standard forms. These records are sent to various central offices where compilations are made and charts are prepared to provide navigators with information in convenient form relative to winds, currents, and temperatures.

Maury's *Physical Geography of the Sea,* published in 1855, was the first textbook or treatise on Physical Oceanography. It was a logical outgrowth of the international program of observations. This pioneer undertaking was followed by great improvements in observational technique, yielding results of high precision, and by the evolution of methods of handling oceanographic observations from early qualitative, descriptive types into a more rigid and quantitative treatment of oceanic phenomena according to the laws of physics. Since the turning point of 1875, marked by James Croll's critical book, *Climate and Time,* dynamical oceanography has progressed at an increasing rate, as indicated by the numerous important papers and the growing list of investigators. All of this progress is intimately connected with the names of Helland-Hansen, V. Bjerknes and V. W. Eckman, whose basic contributions to dynamical oceanography began to appear at the close of the 19th century. During the period since 1900, the great accumulation of observations and the development of more careful reasoning and quantitative treatment of questions pertaining to the solid earth, its atmosphere, and its oceans, led to

greater specialization in the various fields of geophysics, including that of physical oceanography.

Observations without the use of instruments can, in general, reveal but little information concerning ocean surface currents, and nothing about subsurface currents. Direct observations of currents with the aid of floats or current meters require conditions rarely met with in the high seas. For observations even on surface currents we are dependent mainly upon indirect means, such mechanical ones for example as drift bottles set adrift at known positions and times and the departure of ships from their apparent courses. Current measurements are, in general, far from being as precise as observations of other physical phenomena of the ocean water. The most accurate current measurements have been made in shallow inshore waters and, on rare occasions, in deeper waters from specially equipped ships at anchor. As evidence of progress in instrumental equipment attention is called to a late type of current meter designed to rest upon the sea bottom and to record the rate and direction over a period of days. Thus it is now possible to make an intensive current survey of limited regions.

However precise and complete oceanic observations may be, merely making and compiling them is but the first step in the investigation of the ocean. It is only by attempting to coördinate and interpret objective knowledge that we can realize an adequate return from such efforts. This demands the definite formulation and solution of certain special problems suggested by oceanic and other cosmical data with a view to discovering causal relationships between the various phenomena and to showing their harmony with general principles already established by investigations in other fields. Thus by obtaining a series of definite and proved relations, each relation involving two or more types of physical and chemical magnitudes, and by synthesizing these elementary results, we approach the ideal of a system of general laws of the sea, based upon objective knowledge.

In this way we can describe the many diverse phenomena in terms of relatively few and simple fundamental ones. Moreover, attempts to formulate theories of oceanic circulation are no less necessary than observations, even as means of ascertaining the facts about such obscure and complex phenomena.

Biologists, geographers, and geologists have carried on many investigations in physical oceanography, but only incidentally to work on the problems of special interest to them. Accordingly, qualitative and descriptive methods, formerly so characteristic of the sciences of biology, geography, and geology, have been widely used in oceanographic investigations. Such methods and the empirical treatment of quantitative field observations have been very suggestive, have stimulated interest, and have led to certain broad generalizations that are essentially correct, but an early tendency toward rather loose reasoning and lack of consideration of established quantitative principles of physics, resulted in certain erroneous conclusions. Even after both laboratory and mathematical physics were well advanced, inherent difficulties and the incompleteness of field data led the pioneer investigators to varied and conflicting conclusions.

Must we admit that our lack of control and the complexity of such geophysical phenomena render careful reasoning and quantitative treatment impossible of attainment? Although such an achievement may at first appear unattainable let us give the matter due consideration before expressing an opinion. Especially since Croll's contention in 1875 of the necessity of quantitative reasoning in this field, a few scientists have undertaken, by means of a definite formulation of specific problems, to apply mathematics to quantitative ocean data, and thus to contribute to a system of demonstrable principles applicable in general to all similar cases. Such work is essential in developing a science of the sea. Important advances have thus been made and serious errors in certain former conclusions have been

discovered, although in some of the earlier attempts at mathematical applications, significant errors arose from incorrect assumptions and the failure to appreciate characteristics peculiar to such "field" or natural problems. At first, scientists accustomed to problems of laboratory physics attempted to deduce physical laws of the sea from results of laboratory studies and from certain preconceived assumption regarding oceanic conditions. Furthermore, they worked under the disadvantage of having very inadequate data. As more accurate and exhaustive data accumulated, greater attention was directed to interpreting and coördinating field observations than to imposing on the sea purely theoretical laws deduced from laboratory researches.

Prior to 1845, when Professor A. D. Bache, who was then director of the United States Coast Survey, formulated the first systematic plans for investigating currents in the Atlantic and used a vessel equipped especially for that purpose, investigations of ocean currents were merely adjuncts to other work. Actual measurements by means of current meters were first successfully carried out within the modern period and were necessarily restricted to special working conditions. Such measurements and records based upon especially constructed floats have resulted in a detailed knowledge of currents unattainable by the earlier methods alone.

Returning to attempts to deduce laws of oceanic circulation, the German physicist, Zöppritz, was the first to appreciate the importance of the viscosity or internal friction of water in investigations of ocean currents. Before 1878, when he published the first mathematical theory of wind-driven currents, the opinion prevailed that a difference of level was necessary for the production of a current. It was still believed, as stated by Varenius in the seventeenth century, that "water has no natural motion except that which follows when it flows to lower levels." But Zöppritz concluded, from the theory of a viscous fluid, that the wind could produce a surface current by its friction on the

water surface. This upper layer would, in turn, move water underneath, and thus in the course of time give rise to motion throughout the whole body of water between the surface and bottom. In agreement with this, the idea that winds can produce a current by the horizontal force of friction on the surface has always been held by practical seamen. They have always regarded the wind as the main cause of all currents in the open ocean that have an appreciable influence on the navigation of ships. However, when suitable measurements of currents were available, it was found that the results of Zöppritz's theory involving laboratory values of the coefficient of viscosity were of an entirely different order of magnitude from the observed values. Thus the elaborate theory that he developed, although mathematically sound and widely accepted and applied by other investigators, proved defective in practical applications. Yet his work was of value in stimulating further researches and in removing objections previously raised against the theory of wind-driven currents.

Bjerknes and Ekman abandoned the concepts peculiar to *Classical hydrodynamics* for those of *Physical hydrodynamics.* The fundamental assumptions in classical hydrodynamics are: first, flow is either in practically parallel layers as demanded by the idea of viscosity dependent upon laboratory experiments, or fluid friction is neglected; second, the fluid is *barotropic,* that is, it is either homogeneous and incompressible or the density varies only with the pressure. In the atmosphere and the ocean the density varies with factors other than pressure; temperature and humidity influence the density of the air, and the density of sea water depends upon temperature and salinity. Moreover, neither the sea nor the atmosphere can be regarded as fluids flowing only in parallel layers as in the case of controlled laboratory experiments on viscosity. Both have a great apparent viscosity because eddies, characteristic of ocean currents and winds, transfer particles of the fluid across the boundaries between layers along which the aver-

age flow proceeds. Accordingly, laws valid for the ideal fluid of classical hydrodynamics are not adequate for the treatment of problems of the motion of the sea or atmosphere. Professor V. Bjerknes, as a result of collaboration with his father, continued his investigations of fluid motion and founded the science of *physical hydrodynamics* in which the fluid is regarded as *baroclinic,* that is, the density depends not only upon pressure but upon other factors such as temperature, salinity and humidity. He realized that when we cease to consider problems of classical hydrodynamics as special cases of more general hydrodynamical problems, we shall often find new, more fruitful, and general methods of dealing with them. As a matter of fact, in classical hydrodynamics the problems are mainly defined by the surface conditions, while in physical hydrodynamics the necessary conditions are contained in the fundamental equations themselves. Thus the way is prepared for practical generalized methods. His student, V. W. Ekman, devoted special attention to the concept of eddy motion or turbulence and its consequences, and is the first one to include in a rigorous mathematical treatment all of the factors essential to wind-driven currents.

Later contributions of Bjerknes, Ekman, and other investigators inspired by the new viewpoint are widely scattered in many special publications. The need of making these modern results more accessible to students of oceanography is accordingly urgent. For the only adequate attempt to meet this need we are indebted to Dr. Albert Defant, whose book *Dynamische Ozeanographie,* 1929, published in German, presents a connected treatment of the fundamental concepts upon which the modern study of oceanic circulation is based. A glance through this book indicates the extensive use of the methods of mathematical physics that prevails in modern dynamical oceanography. A careful study indicates how well the author has succeeded in unifying the whole subject and in explaining the physical significance of every step in the calculations.

The mathematical-physical treatment of oceanographic problems included in dynamical oceanography may be divided into two parts. First, there is the fundamental part involving the simplest conditions and essential methods. Second, there are modifications of such simple results which are made necessary because of variations in actual conditions, *e.g.*, irregularities in depth. Thus it is possible to provide a sufficiently close connection between ideal problems, essential to any quantitative investigations in oceanography, and actual problems.

It has formerly been necessary to use as a basis for theoretical work such scattered data as the investigator could find. Much has been, and doubtless will be, accomplished in that way. But such a procedure has obvious disadvantages, such as insufficient or unknown precision, incomplete data, or lack of significant factors that may impair or greatly restrict the conclusions. Therefore, it is also necessary to conduct special programs of intensive observation, designed with reference to particular problems, in order to improve and supplement the above more extensive and preliminary type of work. Thus, selection of conditions such as locality or season, and the attempt to observe all relevant phenomena, may afford advantages approaching those of the laboratory physicist who controls the conditions affecting his experiments. For example, serial temperatures observed in the relatively quiet and deep central part of a high-pressure area, undisturbed by land boundaries, provide results corresponding to the simplest conditions and would be of great aid in the study of "normal" temperature gradients. Such observations should also be accompanied by observations on the intensity of solar radiation, turbidity, salinity, and evaporation, and should be continued through different seasons and times of day in order to provide the most important kinds of data. Very little of this intensive type of work, carried on with sufficient continuity and completeness, has been done and it has been restricted to certain portions of small inland seas

or inshore regions. Although results thus obtained are valuable in themselves and as a means of interpreting such fragmentary and widely distributed data as may be available, they cannot take the place of similar intensive work at selected stations throughout the ocean. Actual conditions in typical areas of the great oceans must be carefully observed and studied if any reasonable approach to exhaustive oceanographic investigations is to be realized.

Purely empirical or statistical methods are indispensable in assembling and coördinating various kinds of field data. However, progress from such empirical results toward the ideal of a deductive treatment carried out in accordance with known generalizations of physics is also necessary. Some of the methods of carrying out such investigations will now be briefly outlined.

The Forces that Generate and Maintain Oceanic Circulation

Various forces generate and maintain the motion of the sea. For convenience, they are divided into two groups, *internal* and *external* forces.

Internal forces arise from gravity and the distribution of mass within the sea and are therefore intimately related to its physical constitution which depends upon temperature and salinity. Accordingly, the internal forces are modified by the gain or loss of heat due to radiation, conduction, convection, and evaporation. The addition of fresh water or its removal by evaporation also modifies the internal forces by changing the salinity and the temperature.

External forces include the effect of winds on the sea surface, changes in atmospheric pressure, and the tide-generating force of the sun and moon. These forces cannot directly alter the physical character of the water but may, through the resulting motion, change the distribution of mass and thus disturb the internal system of forces.

In addition to these two groups of *primary* forces there are two *secondary* forces, the deflecting force due to the earth's rotation and the frictional force at the boundaries between water masses moving with different velocities. The first acts at right angles to the motion, to the right of an observer facing in the direction of motion in the northern hemisphere, and to the left of the observer in the southern hemisphere. The second, or internal friction, acts along the line of motion in such a way as to retard the relative motion. Neither of these secondary forces can of itself generate motion. Although both are consequences of motion already generated they exert such an important influence on the circulation as completely to transform it.

The Bjerknes Circulation Theory

The principles of mechanics embodied in Newton's three laws of the motion of a "particle" apply fundamentally to a continuous medium, for example, to a fluid. But in order to investigate fluid motion these principles must be expressed in an appropriate form and the medium must be suitably described.

In 1885 the Norwegian meteorologist, Mohn, estimated the height of the surface of the Norwegian sea at a network of stations by means of serial observations of temperature and salinity. He called this the "density-surface" since its elevation relative to a level surface depends upon the distribution of density. Assuming the current due to the wind to be proportional to the wind velocity and in the direction of the wind, he estimated the shape of the "wind-surface" or surface of the sea necessary to provide a pressure gradient at right angles to the current sufficient to balance the deflecting surface due to the earth's rotation. Adding to this the departures from the mean level of the "density-surface," he obtained the "current-surface" from which the actual current was estimated by using the principle that the deflecting force due to the current balances the force due to the pressure-gradient. Mohn was a

meteorologist rather than a mathematician or laboratory physicist, but was convinced of the necessity of applying quantitative methods in hydrography. His pioneer achievement marks the beginning of a new era in the investigation of ocean currents, and it contributed much toward the development of the more satisfactory modern methods.

The fundamental work of V. Bjerknes in physical hydrodynamics served as a basis for the development by him and his students and colleagues, Sandström, Nansen, Ekman, Helland-Hansen, Sverdrup, and others interested in physical oceanography, of modern practical methods for computing the direction and velocity of ocean currents from serial temperatures and salinities at a network of stations.

The simplest and most commonly used special case of the Bjerknes circulation theorem assumes a fully developed current caused by the distribution of pressure corresponding to the observed distribution of temperature and salinity. A convenient plan for computation has been devised for obtaining the dynamic depth of any point. The dynamic depth equals the work done against gravity in raising a unit mass from that point to the sea surface and is inversely proportional to the density of the water. The deflecting force at a given latitude due to the earth's rotation is proportional to the velocity of the water and must balance the pressure gradient which is proportional to the difference in "dynamic-depth" between the boundaries of the current. In general, his formula expressing these relations serves to calculate the horizontal current at one level relative to that at another level.

Eddy-Viscosity and Turbulence

In the motion of the atmosphere and the sea, instantaneous velocities in the numerous eddies are unknown, only the average velocities are observed or considered in detail. In 1894 Osborne Reynolds demonstrated that in all such cases the coefficient of viscosity must be replaced by the coefficient of eddy-viscosity in order to harmonize theory

and observations. The coefficient of eddy-viscosity or virtual viscosity for the atmosphere is of the order 3×10^5 times the laboratory value for air. For the sea the ratio is of the order 0.2×10^5. Both ratios vary widely with conditions. These great differences between the virtual viscosity and its laboratory value are due to the fact that we do not observe the actual motion in all of its details. Our instruments furnish only mean values averaged over an interval of time and space, while the actual motion is turbulent. Thus turbulent or mixing motion causes an interchange of energy, of motion, and of other properties between the separate fluid layers. In consequence there is a very great apparent viscosity as long as one follows the motion only macroscopically.

Ten years before Taylor, Richardson, and Schmidt published their first papers dealing with turbulence, Ekman recognized and made use of this principle in his pioneer investigations of wind-driven currents. He is the first one to include in a rigorous mathematical treatment all of the factors essential to oceanic circulation that is caused by wind. These factors are the frictional drag of the wind upon the water surface, the deflecting force due to the earth's rotation, and the internal friction due to relative motion within the water. Under certain conditions that are often realized in practice when applying the Bjerknes theory, the internal friction is of little importance and may be neglected. But in wind-driven ocean currents it is always fundamental, and is due to irregular eddies involving an interchange of the position of small masses of water. Turbulence or eddy motion may be measured by the mass of fluid exchanged per unit time per unit area between two neighboring units of volume. This quantity has been called "die Austauschgrösse" by W. Schmidt, and the eddy-viscosity by L. F. Richardson and G. I. Taylor in their investigations of the dynamics of the atmosphere.

Turbulence is important not only in the phenomena of winds and ocean currents as indicated above, but it has a

much wider significance because of the nature of the eddies. An eddy may be defined as a portion of a fluid that moves from a stratum where it has the same physical and chemical properties, *i.e.*, temperature, humidity, salinity, etc., as the fluid surrounding it, to another stratum with which it mixes. Thus, turbulence is the mechanism of heat conduction and diffusion of substances mixed with the atmosphere or the water. It endows a fluid with the ability to transmit heat as readily as a solid having a very large coefficient of conductivity. Likewise a turbulent fluid transmits dissolved substances as readily as an undisturbed one having a very large coefficient of diffusion. The stratification of sea water in layers of increasing density from the surface downward is a fundamental factor tending to prevent mixture or reduce turbulence in proportion to the vertical density gradient.

The assumption of a constant value of the turbulence from surface to the bottom often departs widely from actual conditions. Accordingly each of a series of smaller intervals, within which the turbulence is approximately constant, may be considered separately. Important contributions toward the theory of this more general case and towards the correspondingly complex mathematical procedure have been made by several investigators, notably, V. W. Ekman, J. E. Fjeldstad, K. Hidaka, H. Arakawa and C. G. Rossby. Furthermore, important work involving turbulence has been done by G. Wüst, R. Cherubim, H. U. Sverdrup and others in the Zone of Contact between the sea and air, and between the ground and the air.

The Ekman Theory of Oceanic Circulation

Ekman developed his theory of wind-driven currents by means of the equations of fluid motion derived by Euler and Stokes. Some of the main features may be briefly summarized as follows: Assume a constant wind blowing over a large area of deep water, and neglect the effect of coasts. Because of friction each layer of water tends to

move the one underneath in the direction of flow at the boundary. The resultant force on each thin layer must be zero if velocity is constant. In order that the resultant of the frictional drag above and below such a thin layer may balance the deflecting force due to the earth's rotation, the direction of the velocity in the northern hemisphere must rotate to the right as the depth increases. The magnitude must also decrease. Subject to these simplifying restrictions, a region sufficiently deep and so far away from coasts that their influence can be neglected, Ekman worked out the complete solution of the problem. He found the change in direction to be proportional to the change in depth and the magnitude to decrease as an exponential function of the depth. At the "depth of frictional resistance," D, the direction is reversed and the magnitude is less than four per cent of that at the surface. The value of D depends upon the coefficient of eddy viscosity and the latitude, and is of the order of 200 meters. The surface velocity, V_0 is proportional to the frictional drag of the wind and directed at 45° to the right of the wind in the northern hemisphere. The resultant flow of the whole column of water is to the right of the wind and equals $2/9\ (V_0\ D)$.

A constant gradient due, for example, to a constant slope results in a constant velocity U_0 at right angles to the gradient and of such a magnitude that the deflecting force balances the gradient. Owing to the frictional drag of the bottom a component of velocity develops in the direction of the pressure gradient in the layer of depth D. The flow in the direction of the gradient is $0.16\ (U_0\ D)$ and that perpendicular to it is $0.84\ (U_0\ D)$ within this layer. These three simple type circulations can be so combined that the resultant agrees approximately with actual conditions. For example over the Pacific coastal waters of North America there is a large component of wind directed southward, and parallel to the coast, resulting in a wind current superimposed on a gradient current due to the flow of surface water seaward from the coast. This is replaced by colder

water flowing from lower levels toward the coast and upwelling to the surface.

To make theories of oceanic circulation as serviceable as possible Ekman suggested the following procedure which greatly reduces the difficulty of obtaining a solution corresponding to complicated boundary conditions. First choose a number of simple type problems that serve as a schematic representation of the different conditions of motion in the sea. Solve these relatively simple problems accurately by means of equations of fluid motion. The steady motion computed from the actual differences in density and level in the sea and the actual prevailing winds is identical with the actual one except for trivial periodic variations. In order to calculate the motion from a given density distribution, formulate an elementary type problem corresponding most nearly to it and regard the deduced motion as geometrically similar to the actual. If necessary one can interpolate between two type problems. Then so determine the arbitrary constants in the type problems that the calculated velocities agree approximately with the actual ones.

Calculation of the Velocity of Ocean Currents from their Effect on Temperature Distribution

In the absence of ocean currents the distribution of conservative physical and chemical properties of sea water such as temperature, salinity, and oxygen content may be denoted by the word "normal." The actual distribution of these properties may be regarded as a modification, due to ocean currents, of the normal distribution. Masses of water thus moving along more or less well-defined courses tend to preserve their physical and chemical properties as they move along. By attempting to deduce a circulation that would account for the departures from normal values of one of these properties, velocities of ocean currents can be calculated from their effects without regard to the forces that generate and maintain them.

For example, consider the temperature distribution as an index of circulation. The quantity of radient energy that passes through a unit area of the sea and is absorbed by a thin layer of water varies directly as its thickness and is assumed to be proportional to the amount of radient energy that reaches a horizontal surface. Energy leaves this layer, because of back radiation and evaporation, at a rate approximately proportional to the temperature departure from a constant corresponding to the latitude. The time rate of change of the energy in this volume equals the difference between the rate at which energy enters and leaves. If the water flows horizontally and there is a temperature gradient in line with the direction of the flow, the product of this gradient by the velocity is proportional to the rate at which energy enters or leaves the volume due to the current.

The average of the surface temperatures corresponding to a given circle of latitude tends to approximate to the "normal" for that latitude as the number of observations increases, if they are so distributed as to be representative. An equation has been derived between the temperatures, observed and normal, at two points along a stream line, and the velocity of the current between them. It has been applied to numerous temperature observations made in the North Eastern Pacific, and the estimates of horizontal current velocities agreed well, on the average, with values obtained by other methods. The same principle has also been successfully used to calculate the very much smaller velocity of vertical currents, and in connection with Ekman's theory it served to explain the presence of abnormally cold water off the coast of California and to relate the amount of temperature reduction to the strength of the winds over the ocean. The vertical velocity thus estimated agreed well with that found by the Knudsen equation of the flow of water and salts in a given closed volume. The flow of water out of the volume included the rate of evaporation into the atmosphere.

Typical results for horizontal currents are presented in Table 1 where estimates from wind velocity are arranged in their order of magnitude and corresponding estimates from temperature are entered for comparison. In both methods a steady state of motion and temperature distribution is assumed. On the average the results of the two methods agree, but because of high variability numerous observations are required to obtain precise results.

TABLE 1

OBSERVED WINDS, ESTIMATES IN MILES PER HOUR OF OCEAN-CURRENT VELOCITIES FROM WINDS ACCORDING TO EKMAN'S THEORY, AND FROM TEMPERATURE

												Mean
Wind vel....	18.9	20.0	18.0	8.8	13.0	13.0	11.0	11.0	9.5	7.0	7.0	12.5
Current from wind..	0.62	0.49	0.33	0.27	0.23	0.22	0.20	0.19	0.17	0.15	0.14	0.27
Current from temp.	0.10	0.72	0.09	0.07	0.20	0.34	0.15	0.65	0.10	0.28	0.13	0.26

SUPPLEMENTARY CONSIDERATIONS AND CONCLUSIONS

The instantaneous or synoptical motion in the sea is conveniently represented by *stream-lines* or curves such that the tangent at every point has the direction of the velocity there. Such a representation is called a current field. In general, the inclination of stream-lines to the sea surface is very small and they must satisfy certain conditions: (1) Different stream-lines cannot intersect. (2) They neither begin nor end in the current field except in certain *singular points* and lines.

Stream-lines are continuous curves throughout most of their length, but certain limited parts may have discontinuities. A line towards which two systems of stream-lines converge is called a *line of convergence* (arrows point to-

ward the line). A line from which two systems of stream-lines diverge (arrows point away from the line) is called a *line of divergence.* Again, stream-lines may converge to a point or diverge from a point, or one system of lines may converge to a point and another system may diverge from the same point. In general a line or point of convergence implies a downward flow while a line or point of divergence implies an upward flow in order that the condition of continuity may be satisfied. Such vertical velocities may also result from changes in velocity along parallel stream-lines.

Not only the configuration of the coast lines but also the bottom topography exerts an important influence on oceanic circulation. Neglect at first the effect of compressibility, friction and the deflecting force due to the earth's rotation, and assume that the motion does not vary with time, as denoted by the phrase, "stationary condition." Water adjacent to the bottom of the coast must move along these boundaries. That is, the stream-lines at the boundaries must run parallel to the boundaries. Accordingly, the effect of the accompanying lateral departures of currents from rectilinear courses may extend for a considerable distance from the coast and from the sea bottom. For example, if the bottom departs from a plane in an undulating or wave-like manner the stream-lines of a current flowing across the crests and hollows will have a similar wave-like departure from horizontal lines. The phase and magnitude of these departures depend upon the velocity of the current, depth of the water, and horizontal distance between the crests. A similar distortion of isotherms and isohalines may result, and vertical departures even exceeding 100 meters have been observed. When a layer of light water rests on one of relatively heavy water such disturbances may have little or no effect on the surface although at the boundary they may be greatly amplified. In general the circulation in the sea tends to undulate about simpler average courses.

In his earlier investigations on wind-driven currents Ekman assumed the depth to be uniform, but estimated the influence of the bottom when the depth is less than the "depth of frictional influence," *D*. Later he investigated the more difficult problems of determining the effect of a variable depth on circulation. He found that a uniform increase in depth in a direction parallel to the coast caused a decrease in velocity of the mid-water current as the distance perpendicular to the coast increased, assuming the wind to be parallel to the coast and in the direction of the increasing depth. If the depth varies in an undulating manner, Ekman concluded that the deep-water current would undulate in a horizontal direction, and that the effect is due to the absolute variation of the depth rather than to a variation relative to *D*. The following general principles formulated by Ekman summarize the results. Where the depth increases in the direction of a current a vortex *contra solem* tends to form; where the depth decreases in the direction of a current a vortex *cum sole* tends to form; and where the current follows the direction of bottom contours no disturbance of the current-field results from the bottom topography.

In general, any conditions causing a local accumulation of water in one part of the sea and removal of water from another part will alter the sea surface and thus cause gradient-currents. This alteration will continue until the rates of inflow and outflow balance throughout, assuming winds and other external influences to be stationary. Ekman deduced the general law that a depression of the sea surface causes a rotational motion *contra solem,* and an elevation, a rotational motion *cum sole.*

The practical value of Ekman's theory of stationary wind-driven ocean currents depends partly upon the length of time required for the wind to generate a current velocity approximating its limiting value. His investigation of this difficult problem resulted in the following conclusions, assuming the depth to exceed *D,* and excluding a belt close

to the equator: the time required for a wind-driven current to attain any proportion of its limiting value is independent of the coefficient of friction. The stationary state will be nearly established in a few days' time within about 100 kilometers from the coast and for moderate depths, less than 500 meters (*i.e.*, on the continental shelves). The time increases with the depth of the sea, the breadth of the current, and the latitude, and may amount to several weeks. The surface current is practically established in a few hours' time and responds quickly to changes in the wind. Later estimates by Jeffreys and Proudman agree as to the order of magnitude of the time required for a current to approximate to its stationary value. Accordingly we should, in general, expect a circulation approaching the stationary state to exist and the corresponding theory to apply approximately to actual conditions.

There is, in general, a continual increase of density *in situ* from the surface downward. Any small volume of water lighter than the average at that level tends to rise; if heavier, it tends to sink. Thus the extreme mobility of water results in a stratification according to the density and therefore according to temperature and salinity. Moreover, such stratification from the lightest water at the surface down to the heaviest water at the bottom corresponds to a condition of stable equilibrium in which the center of gravity is as low as possible. A body of water thus stratified resists any force (*i.e.*, frictional drag of the wind) tending to change the relative positions of the different strata. If distorted it tends to return to its equilibrium condition. This resistance to change may be indicated numerically by the amount of work that must be expended in order to transform the stratified body of water into one of equal density throughout.

Earlier investigations of the phenomena of turbulence proceeded on the assumption of an analogy between the behavior of molecules and of eddies. Accordingly, the familiar classical equations of motion, diffusion, and con-

duction were used, in which the coefficients of viscosity, diffusion and eddy conductivity that apply to the laboratory experiments were replaced by new virtual coefficients regarded as constant in each problem. Since the virtual coefficients depend upon the character of the *turbulent motion* rather than on the properties of the substance, water or air, we have reason to believe that they would all have the same values at the same time and place. According to the results of meteorological investigations, nearly the same value of the coefficient has been found for the diffusion of moisture as for the conduction of heat. Less extensive investigations of the ocean have resulted in the same coefficient for the diffusion of salinity as for the conduction of heat. While investigations of the velocity have yielded values of the virtual coefficient of viscosity of the same order of magnitude as those of diffusion and conductivity, significant differences are found. Apparently, the kinetic energy is not carried unchanged by fluid masses as they move across the boundary between two layers. But the same masses appear to carry other properties, temperature, salinity, etc., with but a small change. Although such a procedure adds but little to our understanding of the mechanism of turbulence, in detail, it may lead to equations agreeing very closely with observations. Thus, results may be obtained of fundamental importance in the solution of other problems. Although under certain conditions only very rough approximations result, the agreement between theory and observations may be greatly improved by assuming the coefficient to vary in accordance with the stability, as has been shown by Fjeldstad and others.

Believing that a much more fundamental analysis of turbulence can be worked out which would show more clearly the structure of turbulent motion, several investigators have made encouraging progress specially within the past fifteen years. In these studies detailed measurements of air motion made in laboratories designed to aid in the solution of problems of aviation, as well as atmospheric ob-

servations have contributed essential information. Important parallel advances in the theory of turbulence have been made by G. I. Taylor, O. G. Sutton, L. Prandtl, Th. von Kármán, J. M. Burgers, C. G. Rossby, and others. Owing to the inherent complexity and irregularity of turbulent motion these investigators have necessarily resorted to probability methods. In particular, H. Gebelein, a student of Prandtl, deduced hydrodynamical equations of motion by means of a "probability calculus" as developed in recent years by physicists to solve their problems of "statistical ensembles." The presentation of his results in a volume entitled "Turbulenz' marks a new epoch in this field and shows a distinct advance over preceding theoretical methods. The oceanographer is now furnished with a developing body of information and methods that should impel him to give more attention to the theory of turbulence in the sea, and to make field observations designed to contribute to a better understanding of this important phenomenon.

In conclusion, this survey of the field of dynamical oceanography has shown that there are various indirect methods available for interpreting field observations of winds, temperature, and salinity, and that by means of these methods we can obtain a knowledge of oceanic circulation. It is only by a proper coördination of such methods with suitable direct measurements that so complex a phenomenon as oceanic circulation can be adequately investigated.

LAND MOLLUSKS FROM THE ISLAND OF ROATAN, HONDURAS

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(Communicated by Edwin G. Conklin)

ABSTRACT

Mollusks were collected from Roatan, the largest of the Bay Islands, located about 40 miles off the north coast of the Republic of Honduras. Sixteen species of land shells are here recorded of which four are described as new. The best collecting was among the Pleistocene limestone near the western end of the island. Mollusks previously collected from other of the Bay Islands are reviewed. The relationship is definitely Central American. Those species that do show a more or less distant relationship to the West Indies might have reached the Islands and the Central American Mainland by a Tertiary land bridge connecting Jamaica and Haiti with Nicaragua and Honduras.

OUR knowledge of the land mollusks of the Bay Islands of Honduras has been limited to a paper by Ancey (1886) on a collection from Utilla and a few records from Bonacca listed by Von Martens (1890–1901). Nothing has been known of the mollusks of Roatan, the largest of the Bay Islands, and it therefore seemed desirable to make a collection from this island.

ACKNOWLEDGMENTS

With the aid of a grant from the Penrose Fund of the American Philosophical Society I was able to spend a few days on Roatan in August, 1937, making a study of its molluscan fauna. It is a great pleasure to express my thanks to the Society for the aid which made this work possible. Allen L. Midyette was the field assistant on the trip and has also prepared the sketch map accompanying this report (Fig. 1). We are indebted to the officials of the Standard Fruit Company, in particular Mr. Luigi Bacuzzi of Roatan, for innumerable courtesies during the course of the work.

The study of the material was carried on at the Academy of Natural Sciences in Philadelphia and I am deeply indebted to Dr. Henry A. Pilsbry of that Museum for considerable help in the preparation of this paper. I also wish to express appreciation to Dr. H. B. Baker of the University of Pennsylvania for assistance, particularly in the matter of bibliographic references.

FIG. 1. Sketch map of the Bay Islands of Honduras.

The original set of the material, including types, is deposited at the Academy of Natural Sciences, while a duplicate set is at the Museum of Comparative Zoölogy in Cambridge, Massachusetts.

LOCATION

The Bay Islands (Islas de la Bahia) consist of a small group of islands in the Caribbean Sea from 10 to 40 miles north of the Republic of Honduras. Roatan, the largest of the group being about 30 miles long and 9 miles wide, lies about 30 miles from the mainland.

Roatan is fairly accessible by small schooner from La Ceiba on the mainland and it was from this port that we sailed on

August 11, 1937. Coxen Hole, the largest town on Roatan and the capital of the Bay Islands (Pl. I, Fig. 2), was our headquarters. Our collecting trips were made in different directions from Coxen Hole, and while it was impossible to cover the entire island, we were able to visit representative localities. Upon the completion of the collecting we obtained passage on a small schooner (*El Pato*) for Belize, British Honduras, 180 miles distant, from which port another schooner (*Delphia*) took us to Tampa, Florida.

Topography and Geology

Roatan has a mountainous west to east backbone and reaches a height of about 800 feet near the western end. There are no rivers on the island, merely small streams many of which terminate in dense mangrove swamps. For a brief description of the island together with a detailed account of the archeology see Strong (1935).

The Bay Islands are a seaward extension of the Sierra de Omoa of northwestern Honduras, and consist of ancient crystalline rocks of Paleozoic or Pre-Cambrian age (Schuchert, 1935, pp. 351, 357, 371). The west end of Roatan consists of Pleistocene coral limestone up to 10 feet above the sea. (See Pl. II.) [1]

Ecological Notes

The dense jungle of the interior of the island (Pl. I, Fig. 1) was not very rich in mollusks. However, careful search among the humus yielded a number of species of which *Poteria dysoni*, *Subulina octona*, *Lucidella midyetti* and *Alcadia dysoni jansoni* were the most common. Our best stations for shell collecting were among the limestone outcrops near West End (Pl. II). Here the shells were frequently abundant in the crevices of the rock. *Brachypodella bourguignatiana*, *Microceramus concisus arctispirus*, *Choanopoma andrewsæ roatanensis* and *Streptostyla coxeni* were the most characteristic species of

[1] For further notes on the geology of Roatan see Sapper (1905); Powers (1918); Redfield (1923) and Sapper (1937, p. 108, Pls. 1, 7).

this association. *Truncatella bilabiata* was also present on this limestone, especially near the shore.

Previous Literature on Central American Mollusca

The classic works on the terrestrial mollusks of Central America are those of Fisher and Crosse (1870–1900) and Von Martens (1890–1901). The fauna of Yucatan has been treated by Pilsbry (1891) and more recently by Bequaert and Clench (1933, 1936). A recent paper by the present writer (Richards, 1937) discussed the fauna of Cozumel Island.

The mollusks of Guatemala have been discussed recently by Hinkley (1920) and by Goodrich and Van der Schalie (1937).

The paper by Ancey (1886) mentioned above records 19 species collected by C. T. Simpson from Utilla in the Bay Islands of Honduras. His list is given below. When changes in nomenclature have taken place, the original name is given in parenthesis. The specimens marked with an asterisk (*) have been examined in the Academy of Natural Sciences of Philadelphia.

Euglandina carminensis (Morelet)
(*Glandina carminensis* Morelet)
Streptostyla thomsoni Ancey
Salasiella perpusilla (Pfeiffer)
(*Oleacina perpusilla* Pfeiffer)
Guppya utillensis (Ancey)
(*Conulus utillensis* Ancey)
Thysanophora cæcoides (Tate)
(*Pseudohyalina cæcoides* Tate)
Thysanophora hornii (Gabb)
(*Helix horni* Gabb) [1]
**Microceramus concisus arctispirus* (Ancey)
(*Macroceramis gossei arctispirus* Ancey)
**Brachypodella bourguignatiana* (Ancey)
(*Cylindrella bourguignatiana* Ancey)

[1] Probably a misidentification since *T. hornii* Gabb is restricted to Arizona and northern Mexico.

**Lamellaxis simpsoni* (Ancey)
(*Nothus simpsoni* Ancey)
**Lamellaxis mexicanus utilensis* Pilsbry
(*Nothus mexicanus* Pfeiffer)
**Lamellaxis fordianus* (Ancey)
(*Nothus fordianus* Ancey)
Lamellaxis gracilus (Hutton)
(*Opeas subulata* Pfeiffer)
Poteria dysoni (Pfeiffer)
(*Aperostoma dysoni* Pfeiffer)
**Choanopoma andrewsæ* (Ancey)
(*Cyclostoma andrewsæ* Ancey)
Helicina amæna Pfeiffer
Helicina tenuis Pfeiffer
(*H. lindeni* Pfeiffer)
Alcadia dysoni (Pfeiffer)
(*Helicina dysoni* Pfeiffer)
Pyrgodomus simpsoni (Ancey)
(*Trochetella simpsoni* Ancey)
**Lucidella lirata* (Pfeiffer)
(*Helicina unidentata* Pfeiffer)

Von Martens (1890–1901, pp. 29, 40) mentions the following two species collected by Gaumer from Bonacca:

Alcadia dysoni jansoni (Von Martens)
(*Helicina dysoni jansoni* Von Martens)
Helicina oxyrhyncha Crosse & Debeaux[1]
(*H. rynchostoma* Shuttleworth)

The fauna of the Swan Islands, which lie in the Caribbean Sea about 160 miles northeast of Roatan and 100 miles north of Laguna Caratasca, Honduras, has been studied by Pilsbry (1931). In this same paper is treated the fauna of other smaller Caribbean islands including Old Providence, St. Andrews, and the Cayman Islands.

[1] Ancey (1904) notes that the specimens in the British Museum from Bonacca labelled *H. oxyrhyncha* are not identical with the type specimens of Crosse and Debeaux (1863) the locality of which is unknown. Pilsbry (1927) records *H. oxyrhyncha* C & D. from Panama. Von Martens records *H. rynchostoma* Shuttleworth (= *H. oxyrhyncha* C. & D.) from Colombia and Venezuela.

Land Mollusks from Roatan

Oleacinidæ

Euglandina cumingi Beck

Between Coxen Hole and North Side; between Coxen Hole and French Harbor; West End.

Known from Central America from Yucatan to Panama; also from Venezuela.

Streptostyla coxeni new species (Pl. III, Fig. 2)

The shell is cylindric-oblong with conic spire and relatively acute apex; somewhat glossy. Color unknown as the two specimens are bleached to an extremely pale brown tint. 5½ whorls, the first 2½ smooth, the remaining with sculpture of subregularly spaced axial grooves. Columellar fold moderately strong as in *S. meridina* Morelet.

Length 6.25 mm.; diameter 2.3 mm.; length of aperture 3.3 mm. The type (A.N.S.P. 170020) and one other shell were collected on the limestone outcrops between Coxen Hole and West End.

The species resembles *S. meridina* Morelet of Yucatan and Central America in shape but is smaller and is easily recognized by its conspicuous grooves.

Zonitidæ

Guppya gundlachi (Pfeiffer)

Between Coxen Hole and French Harbor.

Of widespread distribution in Mexico, Central America and the West Indies.

Fruticolidæ

Averellia (**Trichodiscina**) **coactiliata** (Deshayes)

Between Coxen Hole and French Harbor; North Side; West End.

A very common Central American form known from Vera Cruz to Panama.

Urocoptidæ

Brachypodella bourguignatiana (Ancey)

West End (rare).

Hitherto known only from the type locality on Utilla.

Microceramus concisus arctispirus (Ancey)

West End.

This variety has been previously reported only from the Island of Utilla. *M. concisus* (Morelet), which differs very slightly from the variety, is reported from Yucatan, Cozumel, Guatemala, Swan and St. Andrews Islands.

Subulinidæ

Subulina octona Bruguiere

Common at all stations especially between Coxen Hole and North Side.

Widespread throughout all the tropics.

Lamellaxis micra (d'Orbigny)

(*Opeas micra* (d'Orbigny))

Between Coxen Hole and French Harbor; West End.

Of wide distribution throughout tropical America.

Lamellaxis mexicanus (Pfeiffer)

(*Leptinaria mexicana* (Pfeiffer))

Between Coxen Hole and French Harbor; West End.

Known from Mexico, Guatemala and Nicaragua. The Roatan specimens are not *L. mexicanus utilensis* Pilsbry of Utilla Island.

Succineidæ

Succinea recisa Morelet

Between Coxen Hole and French Harbor; West End.

Reported from the mainland of Guatemala, Nicaragua and Costa Rica.

Cyclophoridæ

Poteria dysoni (Pfeiffer)

Between Coxen Hole and North Side.

Reported from the mainland from the State of Vera Cruz to Panama. Ancey found the species on Utilla.

Pomatiasidæ

Choanopoma andrewsæ roatanensis new variety (Pl. III, Figs. 1, 7)

Resembles the typical *andrewsæ* Ancey except that the ribs are somewhat thinner and the shell is slightly more tapering. In some individuals a spiral series of sparse chestnut-brown dots are developed on the lower three whorls. In this respect the shell resembles *C. cozumelensis* Richards, but is distinguished by its thinner ribs and more slender shape.

Type (A.N.S.P. 170018). Length 10.25 mm.; width 6.0 mm.; width of aperture 4.0 mm.

West End, fairly common in crevices in limestone.

C. andrewsæ Ancey is known only from the Island of Utilla. The other related species, *C. cozumelensis* Richards, was described from the Island of Cozumel.

Helicinidæ

Alcadia dysoni jansoni (Von Martens) (Pl. III, Figs. 5, 6)

Common at all stations.

This variety was described from the Island of Bonacca and has not been recorded elsewhere. *A. dysoni* (Pfeiffer) was reported by Ancey on Utilla.

Helicina hondurana new species (Pl. III, Figs. 8, 9)

The shell is depressed, subacutely carinate in the middle, the outlines of the conic spire slightly convex; base weakly convex. Upper surface after the whitish apex is sulphur yellow changing in the last whorl to pale pinkish buff, with two banks of irregular pinkish cinnamon spots near the suture and near the periphery. The base is sulphur yellow; the callus mustard yellow. Aperture subtriangular; the interior yellow brown and pinkish cinnamon above. The lip is nearly white, reflected, produced in a short blunt mucro or projection at the position of the keel. The parietal callus spreads rather far forward, where the edge is indistinct. There is a small tooth at the base of the columella.

Height 8.7 mm.; diameter 12.7 mm.; $5\frac{1}{4}$ whorls.

The type (A.N.S.P. 170019) from West End; also collected from between Coxen Hole and North Side and between Coxen Hole and French Harbor.

The species resembles *H. rostrata* Morelet (from Guatemala) in general appearance but differs from it in being more carinate, more depressed and by having a much blunter projection on the outer lip. *H. denticulata* Pfeiffer, known from San Pedro and Lancetilla, Honduras, is probably also related, but is even more conic. *H. oxyrhyncha* Crosse and Debeaux (from Panama, Colombia, Venezuela and Bonacca (?)) is another mucronate form but can be distinguished by its more prominent sculpture and by the absence of the columellar tooth.

Lucidella midyetti new species (Pl. III, Figs. 3, 4)

The shell is cinnamon rufous, about equally convex above and below, acutely carinate, the keel bearing a rather wide periostracal border which is largely or wholly lost in the adult stage, but often fragments persist especially in the penult whorl where it overlaps the suture of the last whorl. After the smooth embryonic shell it has spiral threads as in *L. lirata* (Pfeiffer), usually about half as wide as their flat intervals, which show minute retractive growth striæ. There are generally about seven above the keel and an equal number or more below the base. The central region of the base is without spiral threads. The aperture is shaped much as in *L. lirata*. The outer margin of the peristome is thin and expanded, the basal margin slightly reflected and a little thickened showing only a very slight thickening or none at the position of the tooth of *L. lirata*. The thin parietal callus is strewn with granules, more conspicuous than in *L. lirata*.

The species is closely related to *L. lirata* but differs conspicuously as follows: It is larger and distinctly more depressed, more prominently carinate; the whorls are flatter; the lip is thinner and reflected less than in *L. lirata*. The presence of the laminæ is also characteristic of this species.

Type (A.N.S.P. 170021). Diameter 5.4 mm.; height 3.2 mm.

Cotype. Diameter 3.2 mm.; height 2.8 mm.

Type locality between Coxen Hole and French Harbor; also collected from between Coxen Hole and North Side and from West End. Common.

Named in honor of my assistant Allen L. Midyette.

L. lirata (Pfeiffer) is a common species of the West Indies, Central America and northern South America (Venezuela). Ancey recorded it (as *Helicina unidentata* Pfeiffer) on Utilla.

Truncatellidæ

Truncatella bilabiata Pfeiffer

West End, living on Pleistocene limestone along the shore.

This species is characteristic of mangrove swamps and brackish water associations. Shells are known from Florida, Cuba, the Bahamas, West Indies, Yucatan, Swan and Old Providence Islands. With the exception of the shells from Yucatan, no specimens are reported from the mainland of Central America.

ZOÖGEOGRAPHY

The mollusca of the Bay Islands may be grouped as follows:

Restricted to the Bay Islands

Streptostyla thomsoni Ancey U
Streptostyla coxeni Richards R
Guppya utillensis Ancey U
Brachypodella bourguignatiana Ancey U R
Microceramus concisus arctispirus Ancey U R
Lamellaxis simpsoni Ancey U
Lamellaxis fordianus Ancey U
Lamellaxis mexicanus utilensis Pilsbry U
Choanopoma andrewsæ Ancey U
Choanopoma andrewsæ roatanensis Richards R
Alcadia dysoni jansoni Von Martens B R
Pyrgodomus simpsoni Ancey U
Helicina hondurana Richards R
Lucidella midyetti Richards R

Primarily of Central American Relationship

Euglandina carminensis Morelet U
Euglandina cumingi Beck R
Salasiella perpusilla Pfeiffer U
Thysanophora cœcoides Tate U

Thysanophora hornii Gabb (?) U
Averellia coactiliata Deshayes R
Lamellaxis mexicanus Pfeiffer R
Succinea recisa Morelet R
Poteria dysoni Pfeiffer U R
Alcadia dysoni Pfeiffer U
Helicina tenuis Pfeiffer U
Helicina amœna Pfeiffer U
Helicina oxyrhyncha Crosse and Debeaux (?) B

Primarily of West Indian Relationship

Lucidella lirata Pfeiffer U
Truncatella bilabiata Pfeiffer R

Of General Tropical American Distribution

Guppya gundlachi Pfeiffer R
Subulina octona Bruguiere R
Lamellaxis micra d'Orbigny R
Lamellaxis gracilus Hutton U

U = Utilla. B = Bonacca. R = Roatan.

The relationship is definitely Central American. With the exception of *Lucidella midyetti* all the endemic species of the Bay Islands are more closely related to mainland forms than to those of the West Indies. The West Indian *Truncatella bilabiata* is of widespread distribution and could easily have been transported by human agency. The genus *Lucidella*, although known from Central America, is more characteristic of the West Indies (Jamaica). Those forms that do show a more or less distant relationship to the West Indies (*Brachypodella, Choanopoma, Lucidella*) might have reached the Islands and the Central American mainland by the Tertiary land bridge connecting Jamaica and Haiti with Honduras and Nicaragua, as discussed by Schuchert (1935, pp. 107–8). This origin of certain elements of the Central American fauna from the West Indies agrees with views expressed by the writer (Richards, 1937) in discussing the fauna of Cozumel.

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PLATE I

FIG. 1. Jungle Trail, Roatan.

FIG. 2. Coxen Hole, Roatan.

PLATE II

FIG. 1. Limestone near West End, Roatan.

FIG. 2. Path in limestone near West End, Roatan.

PLATE III

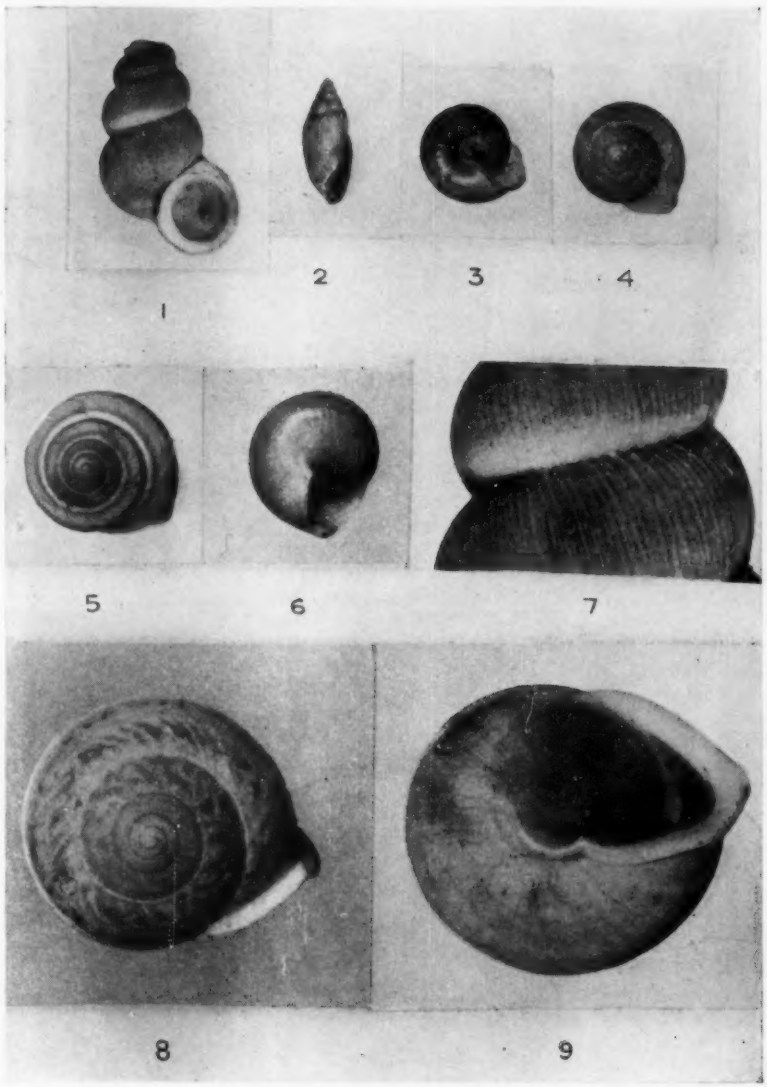

FIG. 1. *Choanopoma andrewsæ roatanensis* Richards. FIG. 2. *Streptostyla coxeni* Richards. FIG. 3. *Lucidella midyetti* Richards. FIG. 4. *Lucidella midyetti* Richards. FIG. 5. *Alcadia dysoni jansoni* Von Martens. FIG. 6. *Alcadia dysoni jansoni* Von Martens. FIG. 7. Detail of *Choanopoma andrewsæ roatanensis* Richards. FIG. 8. *Helicina hondurana* Richards. FIG. 9. *Helicina hondurana* Richards. (All × about 3 except Fig. 7 which is × about 6.)

DISORIENTATIONS OF DEVELOPMENT IN CREPIDULA PLANA PRODUCED BY LOW TEMPERATURES

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(*Read April 21, 1938*)

ABSTRACT

The orientations of the developing egg are caused by vortical movements (plasma flowing) which bring about segregation of structures and substances in the chief axis of the egg and later in the cells into which the egg divides. In normal development these vortical currents run in preëstablished directions and lead to the typical localization of substances and cells in development.

If the developing eggs of the marine gasteropod, Crepidula, are kept at a temperature near freezing for a few hours, plasma flowing and cell divisions are stopped though dividing nuclei may finish division and resting nuclei may grow and prepare to divide. After such eggs are returned to temperatures of about 20°C., plasma flowing is resumed but often in abnormal directions with the result that the directions of division walls and the positions and contents of the cells are abnormal. Sometimes the plasma flowing is resumed at a later stage than that at which it was stopped, with the result that certain early cell divisions are entirely omitted whereas later divisions are normal, except for the reduced number of cells. In short all developmental processes are not stopped at the same time by cold and even though certain stages of development are omitted altogether the development may go forward from a later stage, as if the factors of development were active during refrigeration although the typical expression of those factors is suppressed.

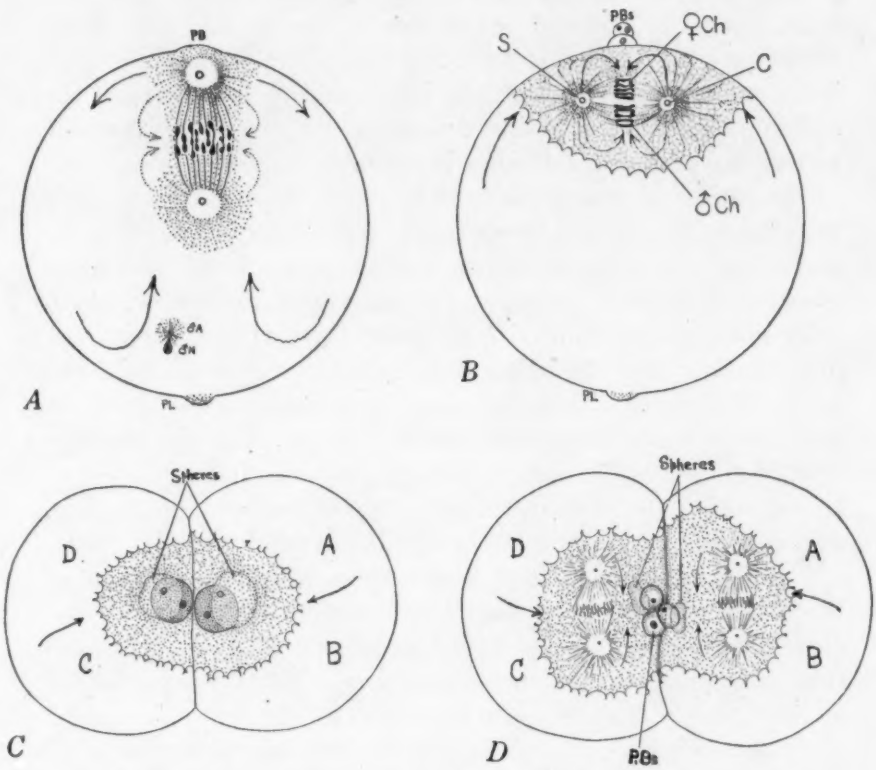
NEARLY forty years ago I called attention to the importance of "Protoplasmic Movement as a Factor in Differentiation" (Conklin, 1899) and three years later I dealt with this subject in a monograph on "Karyokinesis and Cytokinesis" (Conklin, 1902). In both of these papers I showed that definite vortical movements during mitosis in the egg and cleavage cells of Crepidula bring about the typical orientations of mitotic figures and cleavage planes and the differential localizations of different oöplasmic substances. Such vortical movements had been observed by v. Erlanger (1897) and Bütschli (1900) in the eggs of nematodes, but the importance of these movements in the localization of the substances of the egg and in embryonic differentiation had not

been determined previously. Since then such movements have frequently been observed in the eggs of different species of animals, and especially in annelids, mollusks and ascidians.

I. Typical Movements in Maturation and Fertilization

The earliest intracellular movements which I have observed are those which accompany the maturation and fertilization of the egg. In all animals the nucleus of the egg before maturation is relatively large and a large amount of the nuclear contents is set free into the cell body when the nuclear membrane breaks down in the first maturation mitosis. In annelids, mollusks, ascidians and probably in other phyla also vortical movements within the cytoplasm occur coincidently with the escape of this nuclear material. I have studied these movements more particularly in gasteropods and ascidians and in these forms there is movement up through the egg axis to the animal pole and down over the surface toward the vegetative pole (Fig. *A*). The maturation spindle when first formed lies some distance from the animal pole and its axis may lie at any angle with the egg axis, but by means of this vortical movement the hyaloplasm and nuclear contents go up to the animal pole and are there spread out over the surface while the spindle is usually turned into the egg axis and its upper end comes into contact with the surface at the animal pole. If the egg is not fertilized these movements stop permanently at this point, but if a spermatozoön has entered, the movements go on until the maturation spindle is thrust up against the egg membrane at the animal pole and the surface layer is pushed out at this point (Fig. *A*). At the same time the spindle grows shorter and wider as if due to this pressure and finally the first polar body is cut off; the division wall passes through the middle of this shortened spindle, thus giving rise to a very small cell, the first polar body. In similar manner the second maturation spindle is oriented and the second polar body given off under the first.

While the movements accompanying the formation of the first and second polar bodies are taking place, the sperma-



TEXT FIG. I. Typical eggs of *Crepidula plana* showing by arrows the direction of plasma flowings. *A* and *B* viewed from one side, animal pole at top; *C* and *D* viewed from the animal pole. *A*, first maturation mitosis, the flow being up through the center and down over the surface; the mitotic vortices are indicated by small arrows. *B*, first cleavage mitosis, the plasma flowing up over the surface and down in the cleavage plane. *C*, telophase of first cleavage, showing final stage of first cleavage vortex, which was slightly dexiotropic in each cell. *D*, metaphase of second cleavage showing plasma vortex slightly læotropic—*PB*, polar body, *PL*, polar lobe, *C*, centrosome, *S*, sphere, ♂*A*, sperm aster, ♂*N*, sperm nucleus, ♂*Ch*, sperm chromosomes, etc. *A*, *B*, *C*, *D*, positions of future macromeres.

tozoön, which has entered the egg generally in the lower hemisphere, moves into and up through the egg axis, the sperm nucleus being preceded by the sperm aster (Fig. *A*). In

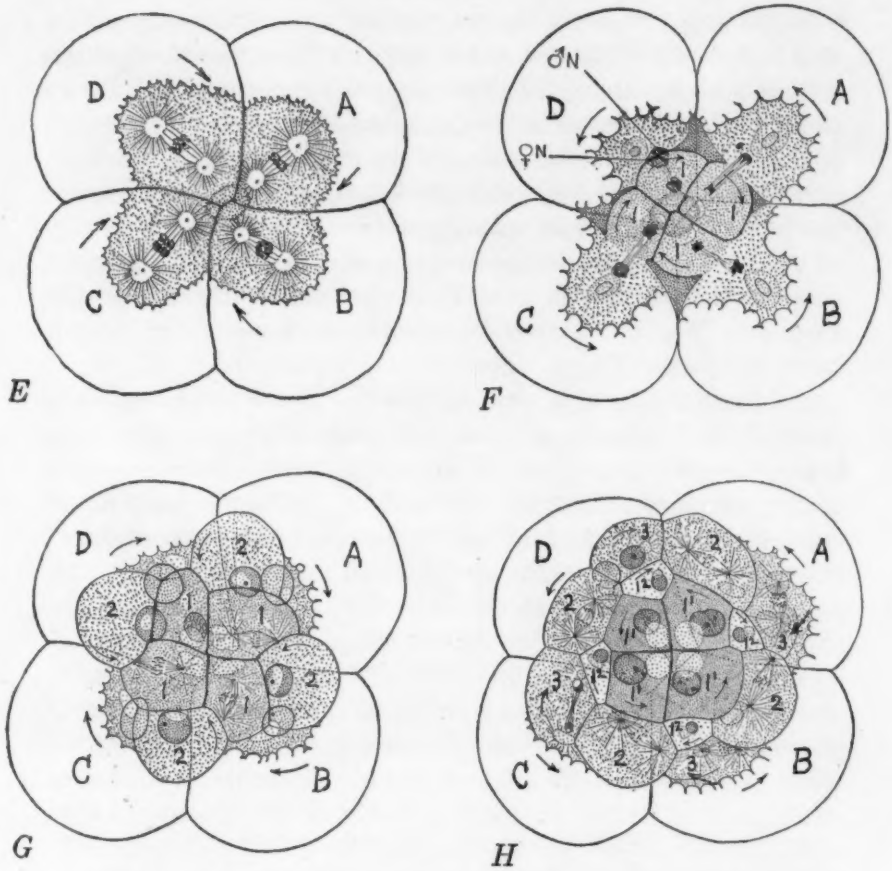
gasteropods this movement continues until the sperm aster and nucleus come into contact with the egg aster and nucleus in the area of clear cytoplasm at the animal pole. At the same time the layer of cytoplasm at the animal pole flows down over the surface toward the vegetative pole, while there is an upward flow through the interior of the egg toward the animal pole. This is plainly a vortical movement, comparable to the movement of particles in a smoke ring.

In ascidians and amphioxus in which there is no egg aster the egg nucleus moves down from the animal pole toward the ascending sperm aster and nucleus until the two nuclei meet near the equator of the egg and on one side of the center, which later development shows to be posterior. It is evident that other factors than the original maturation vortex are involved here. No doubt the sperm aster is a factor, since asters in general are centers of cytoplasmic flowing. But the orientation of these movements accompanying maturation and fertilization is determined primarily by the polarity of the egg, and in ascidians and amphioxus by its bilateral symmetry also.

In Crepidula a path of hyaloplasm is frequently seen at this stage running in the egg axis from the vegetative to the animal pole, and at the vegetative pole there is an accumulation of hyaloplasm which protrudes as a small "polar lobe" at each maturation division, as well as at the first and second cleavages (Figs. *A*, *B*). The animal and vegetative poles are thus clearly marked by the polar bodies and the polar lobe. Protrusion of the surface opposite the poles of the amphiaster is a common phenomenon throughout the cleavage and I have shown elsewhere (1912) that the tension of the surface layer is reduced at these points.

II. Typical Movements during Cleavage

Vortical movements, which are in reverse directions at the two poles of the amphiaster, are very evident during the early cleavages of the egg of Crepidula. I have described these in previous publications (1897, 1902) and in this place I call attention merely to the fact that the location and direction of



TEXT FIG. II. Typical cleavages of *Crepidula plana*, showing directions of vortices in each cell. *E*. Prophase of third cleavage, the plasma flow being in an apical-dexiotropic direction. *F*. Late anaphase of third cleavage, showing the plasma flow in a dexiotropic direction in the micromeres (1) and læotropic in the macromeres (*A*, *B*, *C*, *D*). *G*. Twelve-cell stage; the macromeres have given off the second set of micromeres (2) in a læotropic direction and the plasma flow in these micromeres is also læotropic, while it is dexiotropic in the macromeres. The first set of micromeres is dividing in a læotropic direction. *H*. Twenty-cell stage; the first set of micromeres has divided læotropically into the central cells (1^1), in which the plasma flow is læotropic, and the turret cells (1^2), in which the flow is dexiotropic. The second set of micromeres (2) are dividing læotropically and the macromeres have just given off the third set (3) in which the flow is dexiotropic, while it is læotropic in the macromeres.

mitotic figures and cleavage planes are determined by these vortical movements of the cell contents; also the segregation and isolation of different substances in the different cleavage cells is brought about by these same movements. The directions of these vortical movements during the early cleavages of Crepidula are indicated in the accompanying figures *C–H*, in which it will be seen that at each cleavage the vortices at the poles of the amphiaster are greater on the animal pole side of the cell than on the opposite side, and that there is a spiral twist of the vortices of each cleavage, that of the first being clockwise (Fig. *C*), the second counter-clockwise (Fig. *D*), the third clockwise (Fig. *E*), etc.

In typical cleavage of Crepidula the first four blastomeres are approximately equal; from these, three quartets of micromeres are separated which subdivide and give rise to the entire ectoderm (Figs. *G*, *H*, 1, 2, 3); a fourth micromere (mesomere, 4*d*) is formed from the left posterior macromere *D* and gives rise to practically all the mesoderm; while the remaining portions of the macromeres constitute all of the endoderm. Thus at the 25-cell stage the three germinal layers are segregated in 20 ectoderm, 1 mesoderm and 4 endoderm cells. It is important to recall these well-known facts of the normal cleavage and differentiation in Crepidula, in order to appreciate the changes induced by experiments.

III. Experimental Modifications of Mitotic Movements

Intra-cellular movements accompanying maturation, fertilization and cleavage of the egg may be altered or stopped in many ways,—by pressure, centrifugal force, lack of oxygen, anæsthetics, hypertonic solutions, cold, etc.,—but the actual causes of these movements, like the real causes of amœboid, ciliary or muscular contractility, are largely unknown. These vortical movements are usually very slow and can be seen only indirectly by comparing successive stages in development, although in nematodes and ascidians they are sometimes rapid enough to be seen directly in living eggs.

The present paper deals with the effects of cold on these oöplasmic movements during maturation, fertilization and cleavage in the eggs of *Crepidula plana*. Attention was first called to these effects by the remarkable disorientations of mitotic figures and cleavage planes in eggs which had been kept over night in a refrigerator in order temporarily to stop development. Following this, a long series of experiments was undertaken to determine (1) the most effective temperatures and stages for causing these disorientations, (2) the manner in which they are produced, and (3) the effect of such disorientations on later development.

1. *The most effective temperature* was found to lie between − 1° and + 5° C. for a period of from 2 to 20 hours or more; some eggs indeed were kept at a temperature of 5° C. for 3 days without destroying their ability to divide when returned to water of about 20°. All mitotic figures and division planes from the first maturation division to the 8-cell stage, or even later, may be disorientated by cold; intermitotic stages and late cleavages are affected relatively little. This is significant as indicating that it is the movement accompanying mitosis and cell-division that is chiefly affected by cold.

2. *Modifications of Cytoplasm and Nuclei While Still in Cold.*—The most evident direct effect of cold treatment is to cause the hyaloplasm throughout the egg to clot or contract into islands, which are often connected by strands. A principal strand, which is usually present before the first cleavage, runs from the animal to the vegetative pole along the egg axis (Figs. 2, 3, 5, 9). Other strands or lines of islands may be roughly radial around the asters or centrospheres (Figs. 2, 6, 46, 48). Associated with this clotting there is evidently a loss of water from the cell since the cell membrane is usually wrinkled or irregular when the clots are most evident (Figs. 2, 6, 47). On return to room temperature the eggs regain their smooth outlines and the clots disappear in a short time ($\frac{1}{2}$ to 2 hours).

Eggs with nuclei in the resting stage that have been kept for 8 or more hours near 0° C. have enormously large nuclei

and nucleoli, thus showing that resting nuclei may continue to absorb fluid from the cell body at these low temperatures (Figs. 48–51, 64). Centrospheres also may be very large in such eggs (Fig. 6 sp., 48, 56, 64) but it is often difficult to distinguish centrospheres from cytoplasmic islands or clots (*e.g.* Figs. 46, 47, 56). Chromosomes may form rings, masses or equatorial plates within such enlarged nuclei (Figs. 50, 51, 56) or if the nuclear membrane has dissolved in the prophase the nuclear contents may elongate in the form of an abnormal mitotic figure (Figs. 52–55) which is frequently disoriented.

3. *Effects of Gravity on Eggs Subjected to Cold.*—Crepidula eggs do not orient with respect to gravity and under normal conditions cytoplasm collects at the animal pole and yolk at the vegetative irrespective of the relation of the egg axis to the direction of gravity. But when eggs are left undisturbed in the refrigerator for long periods during which the normal vortical movements are stopped the lighter cytoplasm and heavier yolk may be partially separated in the direction of gravity, as in Figs. 1, 6, 9, 10, 14, 62, 63, 87. This separation is similar in effect to that caused by weak centrifugal force. Refrigerator eggs in the 2-cell stage often have their cytoplasm carried down into the cleavage furrow toward the vegetative pole (Figs. 46, 52–55), probably owing to the long-continued action of gravity on eggs with the vegetative pole up. Sometimes cytoplasm and nucleus are lodged against the first cleavage furrow in one cell and carried away from it in the other (Figs. 53, 54); this is the condition found in eggs which have been centrifuged in a direction at right angles to the cleavage plane. There can be little doubt that the action of gravity for several hours on eggs in which normal cyclical movements have been stopped by cold does cause the heavier yolk to go down and the lighter cytoplasm and nuclei up, and this is especially true of eggs which have been injured or killed.

4. *Modifications of mitosis and cell division after return to room temperature* are found chiefly in the disorientation of amphiasters and cleavage planes. Within half an hour after eggs are returned to a temperature of about 22° C. all clotting

of cytoplasm disappears and mitotic figures become more sharply defined, and they are frequently, but not invariably, turned out of their usual positions (Figs. 10, 11, 25, 37–45, 58–63, *et seq.*). This disorientation of spindles and cleavage planes is the most striking after effect of cold treatment. It is evidently caused by modifications of the normal cytoplasmic movements accompanying mitosis. Study of the normal vortical movements in cell-division long ago convinced me that the spindles are turned into their usual positions by such currents (Conklin, 1902) and later work on the effects of hyper- and hypo-tonic solutions, or lack of oxygen in the water, or especially dislocations by centrifugal force confirmed this conclusion (Conklin, 1912, 1917). The present work on the effects of cold are strikingly like those produced by hypertonic solutions. In both there is a loss of water from the cell and increased viscosity of the cytoplasm; in both the normal vortical movements of the cytoplasm are modified or suppressed and cell division is stopped. In Crepidula neither x-rays, radium, ultraviolet, nor electric current of 200 mil. amps. acting for as long as three hours has any appreciable effect on these movements and orientations.

When nuclear division is stopped by cold it is resumed when eggs are returned to room temperature but when division of the cell body has been stopped that division is never resumed. When the first cleavage has been stopped two nuclei are left in the single cell body (Figs. 11–13). On return to room temperature these nuclei may then resume division (Figs. 14, 15) and if eggs were subjected to $-1°$ for about six hours, the typical second division into four macromeres may also be omitted, and two micromeres (ectomeres) may be formed at the second cleavage (Fig. 16).

5. *Changes in the Number of Blastomeres with No Changes in Differentiation.*—In typical eggs there are always four equal macromeres, which give rise to three quartets of ectomeres, but in eggs in which there is only a single macromere, although this usually contains two nuclei which are the products of the first cleavage mitosis (Fig. 13), the ectomeres are formed in duets instead of quartets.

Typically, mitotic figures and the resulting cleavage planes alternate in direction in successive cleavages; the rotation of cell contents and nuclei is clockwise in the first cleavage, counter-clockwise in the second, and so on through many cleavages. When both first and second typical cleavage furrows are suppressed the single macromere, with two nuclei, gives rise to three duets of ectomeres in the typical directions, the first set coming off in a clockwise direction, the second counter-clockwise, etc. Even the subdivisions of these duets may be in the typical direction as shown in Figs. 13–24. In some eggs one of these macromere nuclei divides a fourth time, well in advance of the other one, and gives rise to a large cell which corresponds in position and general appearance to the mesomere, 4*d*, of typical eggs (Figs. 23, 24).

Other cases in which there is a single macromere with one, two, or more nuclei are shown in Figs. 25–36. When the first cleavage spindle is turned into a position oblique to the egg axis protoplasmic micromeres may be formed at the animal pole but these are always abnormal and they never continue to develop (Figs. 26, 27). Other eggs with two nuclei in the single macromere give rise to ectomeres that are more or less abnormal in position or time of division (Fig. 28) while in others the macromere nuclei after giving rise to micromeres may subdivide into four, eight, or many nuclei without division of the macromere itself (Figs. 33, 35, 36). In all of these cases the differentiation of ectomeres and mesomeres from the entomere (macromere) depends upon the segregation by protoplasmic movement of different substances in the polar axis of the egg and the sizes and positions of cells depend upon cytoplasmic currents which turn the amphiasters into typical positions.

A rather common disorientation is one in which the first cleavage spindle is turned into the chief axis of the egg and the resulting cell division is transverse to that axis (Figs. 37–45). Such eggs rarely segment a second time, as in Fig. 45, and they rarely develop further, although Fig. 36 may be such a case. It is interesting to see that in such cases the

nucleus, centrosphere and cytoplasmic area in the lower cell rotate in the telophase into a position on the animal pole side of the cell (Figs. 38–40). The fact that such eggs do not generally give rise to ectomeres, as in the cases previously described, indicates that processes of differentiation are more seriously disturbed by an equatorial first cleavage than by the suppression of the typical first and second cleavages.

Eggs which are in the two-cell stage when exposed to cold show many instances of clotting of cytoplasm, disorientation of mitotic figures and cytoplasmic areas, as shown in Figs. 46–63. These cytoplasmic and nuclear changes have been described in the previous section and I take up here the later cleavages of such eggs. Of course, many such cleavages are very abnormal, especially when the temperature was 0° or lower or the time of exposure was prolonged. Some of these abnormal cleavages are shown in Figs. 65, 82–89, and any general principles illustrated by them will be discussed later. On the other hand many cleavages are perfectly normal, except that the second typical cleavage is omitted altogether (Figs. 66–75). This is proved not only by the size and differentiation of the daughter cells but also by the direction of the divisions by which they are formed. In the typical second cleavages into four macromeres the amphiasters are always læotropic in position (Fig. *D*), but in these eggs the amphiasters are dexiotropic as they are in the typical third cleavage (Figs. 66, 67). The second typical cleavage is entirely omitted in these eggs, consequently there are only two macromeres, and the micromeres are formed in twos instead of fours, but in every other respect they are entirely typical. The contents, positions and subdivisions of these cells are precisely as in typical cleavage, except that since they are only half as numerous they are twice as large. The first set of micromeres (1) comes off in a clockwise direction (Figs. 66, 67); the second (2) counter-clockwise (Figs. 68, 69, 72); the third (3) clockwise (Figs. 71, 73–75). The first set subdivides unequally and in a counter-clockwise direction into a larger central (1^1) and a smaller peripheral "turret" (1^2)

cell (Figs. 69–74), and the central cells then divide in a clockwise direction (Fig. 75); the second set subdivides in a clockwise direction (Figs. 73, 74) in a wholly typical manner, and one of the macromeres then gives rise to the mesomere, 4*d*, in a counter-clockwise direction (Fig. 76). In all these cells, during the telophase and resting stage, the centrospheres turn into a position on the animal pole side of each cell (Fig. 67) and the coarse granules over the upper aster of each mitotic figure are precisely as they are in typical cleavage (Fig. 68).

All of these cases indicate that the organization of the egg and of different blastomeres precedes cell division and that differentiation does not depend upon a particular succession of cleavages nor the number of blastomeres. Furthermore the segregation, localization and orientation of cell contents and of mitotic figures is caused by regular vortical movements in the cytoplasm.

Of course the disorientation of these movements by low temperature frequently leads to very abnormal forms of cleavage but such abnormalities only confirm the conclusions of the preceding paragraph. Divisions of the cell body may be suppressed following the division of the nucleus leaving a binucleate or multinucleate macromere, the later divisions of which give rise to abnormal numbers of micromeres (Figs. 77–79), or the typical number of macromeres may be increased (Fig. 80) or decreased (Fig. 81) without altering the typical form of cleavage and differentiation, although the total number of cells is altered.

Figures 82–89 are a few of the abnormal forms of cleavage caused by low temperature. In these the order of some of the cleavages is indicated by Roman numerals and it is evident that profound disorientations of development have been caused. In some of these the segregation of cytoplasm toward the animal pole of each cell and of yolk toward the vegetative pole is evident; in others even this differentiation has been disturbed or disoriented in some of the cells, but none of these are inconsistent with the general conclusion already expressed that the disorientations of development

caused by cold are the results of modifications of the typical protoplasmic movements within cells. The probable interpretation of each of these figures 82–89 is given in the description of figures.

6. *The Later Development of Eggs Subjected to Low Temperatures.*—The development of many of these eggs was followed for as long as ten days and until larval organs had appeared. Some of those in which the second cleavage had been omitted, but the form of later cleavages was typical, except for the number of blastomeres, gave rise to normal blastulæ, gastrulæ and veligers. In some the gastrulæ were elongated, but otherwise normal. Many gave rise to exogastrulæ with the shell gland, velar cells and œsophagus well formed in the ectodermal hemisphere. Partially separated blastomeres of the two-cell stage developed abnormally. Some small gastrulæ and veligers were apparently normal and may have come from eggs in which the first cleavage was equatorial and the vegetative half was completely cut off. In other experiments small fragments of larvæ with differentiated ectoderm cells were formed, evidently from isolated blastomeres. However, such isolated blastomeres were not followed individually through their development, but there is no reason to suppose that a fragment of an egg may give rise to an entire larva. On the contrary many previous experiments have proved that isolated blastomeres of Crepidula are not totipotent.

Conclusions

The results of this study may be summarized as follows:

1. Fertilized eggs of *Crepidula plana*, before cleavage or in its early stages, when subjected to temperatures of from − 1° to + 5° C. for from 2 to 20 hours show the hyaloplasm contracted into islands or clots; typical vortical movements of the cell contents are stopped; resting nuclei may continue to grow by absorption of fluid from the cytoplasm.

2. If eggs are left undisturbed in the refrigerator for 6 or more hours the heavy yolk tends to settle to the lower side

and the lighter cytoplasm to rise to the upper side, but this segregation is never complete. This partial segregation is probably due to the cessation of vortical movements since it does not take place at usual temperatures when such vortical movements are present.

3. Low temperatures do not affect all processes of cell division and differentiation equally or simultaneously. The clotting of hyaloplasm, probably accompanied by loss of water, is the first visible effect. This is associated with partial or complete suppression of vortical movements in the cell and the stoppage of cytodieresis. Then follow the disappearance of spindle fibers, asters and centrosomes and the suppression of division of the chromosomes. If mitosis had reached the telophase before subjection to cold, the daughter chromosomes do not form chromosomal vesicles but lie unchanged in a mass of archiplasm occupying the area of the former amphiaster. Resting nuclei and nucleoli continue to grow in the cold and may become enormous. All of these modifications are similar to those caused by hypertonic solutions (Conklin, 1912).

4. Recovery from these effects, when the eggs are returned to a temperature of *ca.* 22° C., is in the following order:

(1) Disappearance of cytoplasmic clots. (2) Reappearance of amphiasters and resumption of nuclear division. (3) An interrupted cell division (cytodieresis) is never completed, but is entirely omitted; thus binucleate cells arise. (4) The typical differentiations of blastomeres are more or less independent of their number and sequence, but not of their orientation with respect to the egg axis. After the first or second cleavage furrows have been suppressed they are never completed, and when the vortical currents reappear they and the orientations of the mitotic figures may be those typical of the third and all later cleavages and in that case typical micromeres (ectomeres) are formed in sets of two (duets) instead of sets of four (quartets). However, if the renewed vortical movements are atypical and the mitotic figures are turned out of their typical positions the resulting

cleavages and differentiations are atypical. (5) These results indicate that various processes in cell division and differentiation may be halted by cold at different times and thus their typical correlations altered. They also indicate that after the orienting movements of an earlier stage have been stopped for a time they are not resumed at the point where they were stopped, but are followed by vortical movements characteristic of a later stage, as if the factors of typical development were undergoing progressive change during the period of refrigeration, although the typical expression of those factors is suppressed.

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DESCRIPTION OF FIGURES

All figures are of whole eggs and cleavage stages of *Crepidula plana*, which had been subjected to low temperature (− 1° to + 5° C.) for from 4½ to 24 or more hours. They were then either fixed at once and then stained and mounted entire, or were kept in sea water at room temperature (ca. 22° C.) for periods varying from 1 to 24 hours before being fixed, stained and mounted.

All figures were drawn with the aid of a camera lucida under Zeiss 3 mm. oil immersion, ocular 4, at table level (magnification *ca.* 550 dia.); in the process of reproduction they have been reduced by about ⅓ so that they are magnified about 350 dia. as here represented.

In all eggs that were kept at − 1° to + 5° C. for one hour or more and then fixed at once, the cytoplasm throughout the yolk area is clotted or contracted into islands and strands; this clotting disappears within an hour when eggs are brought back to room temperature, but cytoplasm and mitotic figures are frequently dislocated from their normal positions. It should be emphasized that the eggs and cleavage stages shown in these plates are only a few of the innumerable forms of abnormalities caused by cold. The figures selected for reproduction here are those which could be most readily interpreted in terms of normal development.

PLATE I

FIG. 1. Egg in first maturation metaphase with the upper aster attached to the surface at the animal pole, which is drawn in at this place. The unusually thick layer of peripheral cytoplasm over the vegetative half probably indicates that this side was uppermost while in the cold chamber; islands and strands of cytoplasm are found throughout the yolk; the sperm nucleus (♂*N*) lies near the equator in a strand of cytoplasm leading from near the vegetative pole to the maturation spindle; 0° for 6 hrs., fixed at once.

FIG. 2. Metaphase of second maturation division, spindle tangential to surface; sperm nucleus (♂*N*) at periphery near equator, no sperm aster evident; clots of cytoplasm in the yolk and a central line of such clots through the axis of the egg; the egg surface is wrinkled, indicating that the egg has lost water; − 1° for 8 hrs., fixed at once.

FIG. 3. Anaphase of second maturation (2d mat.), spindle greatly elongated; sperm nucleus and aster (♂*N*) below equator; polar lobe (*PL*) at vegetative pole, from which an axial path of cytoplasm runs up to the maturation spindle; clots of cytoplasm throughout the yolk; − 1° for 6 hrs., fixed at once.

FIG. 4. Telephase of second maturation; egg chromosomes and aster (♀ *Ch* & *A*) in a semi-circular group at the animal pole; second polar body under the first, which has divided; sperm nucleus and aster widely separated near center of egg; polar lobe (*PL*) at vegetative pole; − 1° for 6 hrs., then room temp. 22 hrs.; development permanently halted.

FIG. 5. Resting egg nucleus at animal pole; sperm nucleus (♂*N*) in center in a path of cytoplasm leading from the animal to the vegetative pole, the latter marked by the polar lobe; no clots of cytoplasm in the yolk; − 1° for 6 hrs., room temp. 1 hr.

FIG. 6. Egg kept at 1° C. for 52 hrs. and then fixed at once. The principal mass of hyaloplasm (*Hy*) and the chromosomes of the egg nucleus (♀ *Ch*) are near the vegetative pole, probably owing to the long-continued action of gravity on an inverted egg; sperm nucleus (♂*N*) and spheres (*Sp*) near equator; cytoplasmic clots and strands are roughly radial.

FIG. 7. Giant second polar body containing two karyomeres, first polar body dividing; large egg nucleus (♀*N*) and aster (♀*A*) at the animal pole and an equally large sperm nucleus (♂*N*) and aster (♂*A*) at the vegetative pole; − 1° for 6 hrs., room temp. 22 hrs. Movements of nuclei and asters and formation of cleavage spindle have been stopped.

FIG. 8. First cleavage spindle nearly vertical in the egg axis, which is marked by the three polar bodies (*PBs*) and the polar lobe (*PL*); 0° for 6 hrs., room temp. 1 hr., during which time the cytoplasmic clots have disappeared, but the first cleavage spindle has not turned into its normal position at right angles to the egg axis.

FIGS. 9 and 10. Eggs of the same stage and experiment as Fig. 8, but in which a thin layer of cytoplasm covers the vegetative half, presumably because of the slow action of gravity on eggs which had the vegetative pole uppermost. The first cleavage spindles are oblique to the egg axis, owing to the interruption of orienting currents in the cytoplasm; 0° for 6 hrs., room temp. 6 hrs., during which the cytoplasmic clots have disappeared.

FIG. 11. Anaphase of first cleavage spindle, which is in normal position; the thickened layer of cytoplasm at the animal pole indicates that this side was uppermost while the egg was in the cold; clots of hyaloplasm (*Hy.Cts.*) in the yolk; − 1° for 8 hrs., then fixed at once.

FIG. 12. Telophase of first cleavage, in which the daughter chromosomes have formed vesicular nuclei, but no indication of cell division is present; the spindle remnants are unusually long at this stage and clots of cytoplasm are found in the yolk; − 1° for 8 hrs., then fixed at once.

PLATE I

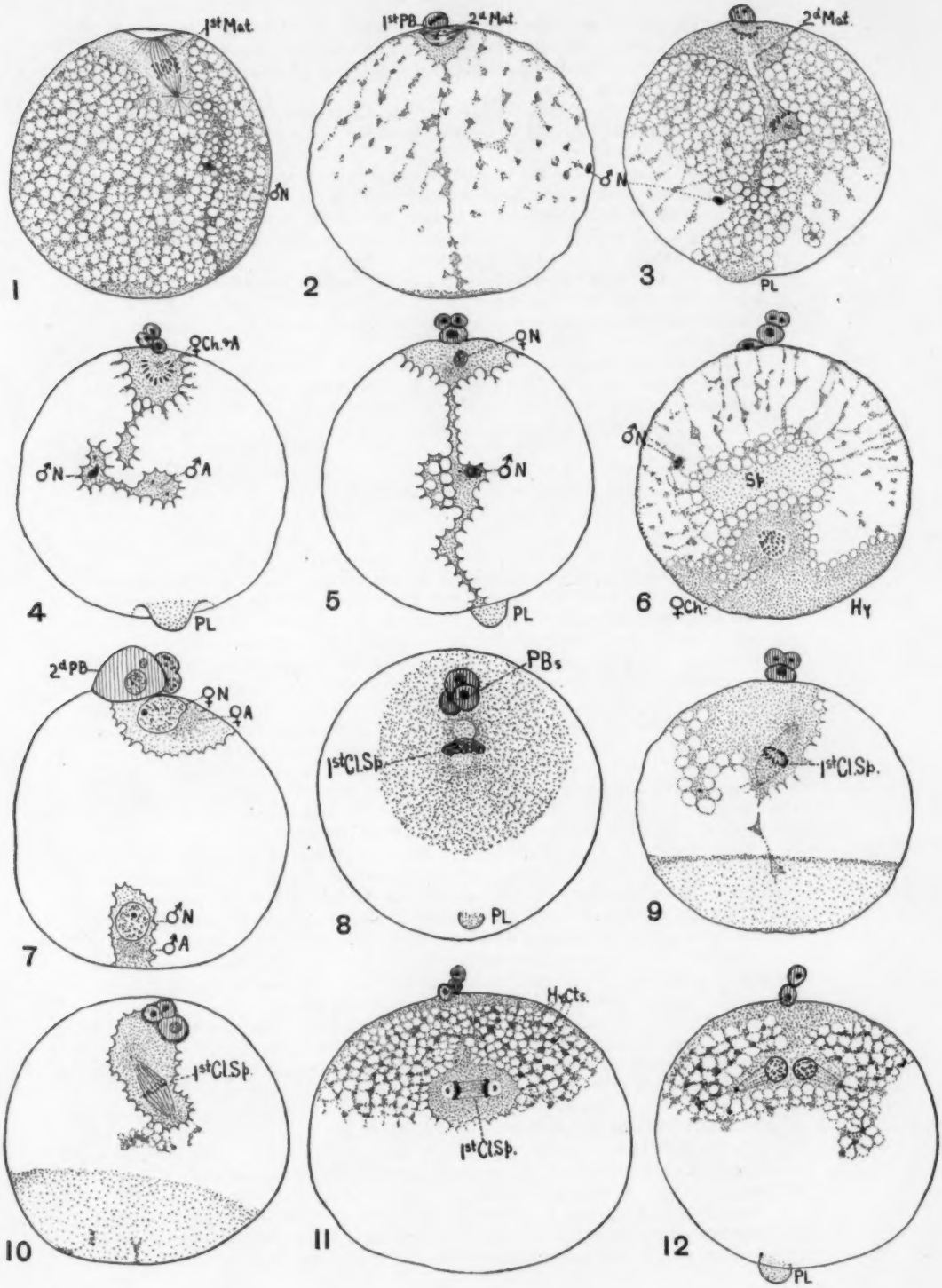

1st Mat.
♂N
1
1st PB.
2d Mat.
♂N
2
2d Mat.
♂N
PL
3
♀Ch.+A
♂N
♂A
PL
4
♀N
♂N
PL
5
♂N
Sp
♀Ch.
Hy
6
2d PB.
♀N
♀A
♂N
♂A
7
PBs
1st Cl. Sp.
PL
8
1st Cl. Sp.
9
1st Cl. Sp.
10
Hy. Cts.
1st Cl. Sp.
11
PL
12

Plate II

Fig. 13. First cleavage furrow suppressed, but its position (1st *Cl.P*) indicated by a deeper staining line of cytoplasm; daughter nuclei (*N*) and asters (*A*) enormous, showing that cold does not stop their growth; cold room (*ca.* 0°) for 6 hrs., room temp. 4 hrs.

Figs. 14 and 15. First cleavage furrow suppressed, but indicated by line of cytoplasm; second cleavage spindles present but not in normal positions; same preparation as Fig. 13.

Fig. 16. Egg in which the first cleavage furrow was suppressed, but the nucleus divided, leaving both daughter nuclei (*M*) in the one macromere. At the second cleavage each of these nuclei divided, giving rise to two micromeres of the first set (1) which are not in contact; connected with each of the four nuclei is a centrosome (*A*); − 1° for 6 hrs., room temp. 6 hrs.

Figs. 17–21. Same preparations as Fig. 16, showing formation of micromeres in sets of twos from the single macromere containing two nuclei. In Fig. 17 the first set of micromeres (1) has sub-divided in normal manner into a larger central (1^1) and a smaller peripheral "turret" cell (1^2) as indicated by the bonds; the micromeres of the second set (2) have come off in a læotropic direction, as in normal eggs, and the spindles shown in dotted outline at a lower level in the macromere are dividing to give rise to two micromeres of the third set.

Fig. 18. All three sets of micromeres have been given off from the single macromere with its two nuclei (*MM*); the dotted transverse line indicates the position of the suppressed first cleavage furrow. The two nuclei in the macromeres are labelled *M* and lie under the spindles in the central cells (1^1); the latter are now dividing a second time, having given off the "turret" cells (1^2) at their first division, as indicated by the bonds. The second set of micromeres (2^1, 2^2) have divided normally, as indicated by the bonds, and those of the third set (3) have formed in the normal direction, as shown by the bonds. All of these micromeres are in sets of twos, since the typical first and second cleavage furrow were suppressed and the two daughter nuclei of the first cleavage are left in an undivided egg cell, while the second typical cleavage into four macromeres is omitted entirely.

Fig. 19. Another egg from the same slide as the preceding, showing the micromeres of the first, second and third sets, perfectly normal in relative positions, but approximately twice the size of normal micromeres since they are present in twos instead of fours, owing to suppression of the first and second cleavage furrows and the presence of two nuclei (*M*) in the undivided macromere. The first set of micromeres (1) divided first into an apical (1^1) and an outer "turret" cell (1^2), as indicated by the bonds, and the apicals are now dividing again in typical dexiotropic direction ($1^{1.1}$, $1^{1.2}$); the second set (2) has subdivided in læotropic direction (2^1, 2^2), while the third set (3) has not subdivided.

Fig. 20. A side view of an egg from the same slide as the preceding; the two nuclei in the macromere (marked *M*) are shown in dotted outline under one micromere of the third set (3), the spindle on the left is in a micromere of the second set (2), and the two cells on the right (2^1, 2^2) are also of the second set; the remaining six micromeres are of the first set, as indicated by the labels.

Fig. 21. Apical view of an egg similar to Fig. 20. The micromeres of the second (2) and third (3) sets meet in a polar furrow under those of the first (1) set, the subdivisions of which are indicated by bonds and labels.

Fig. 22. Side view of an egg like Fig. 19 with 1 macromere containing two nuclei (*M*) and 12 micromeres; the first, second and third sets of micromeres were formed with 2 cells in each set, but the first set has subdivided twice and the second set once; − 1° for 6 hrs., 22°, 16 hrs.

Fig. 23. One macromere with 2 nuclei (*M*) and 16 micromeres in three sets of two each, which have subdivided so that there are 6 of the first set, 8 of the second set and 2 of the third set, which latter are shown dividing; in addition there is 1 large cell of the fourth set corresponding to the mesoderm cell (4*d*) of typical cleavage − 1° for 4½ hrs., 22°, 24 hrs.

Fig. 24. Egg similar to preceding but with a faint indication of a first cleavage furrow between the two macromere nuclei (*M*); there are 16 micromeres (only 12 are shown) and one mesomere (4*d*); − 1° for 6 hrs., 22°, 16 hrs.

PLATE II

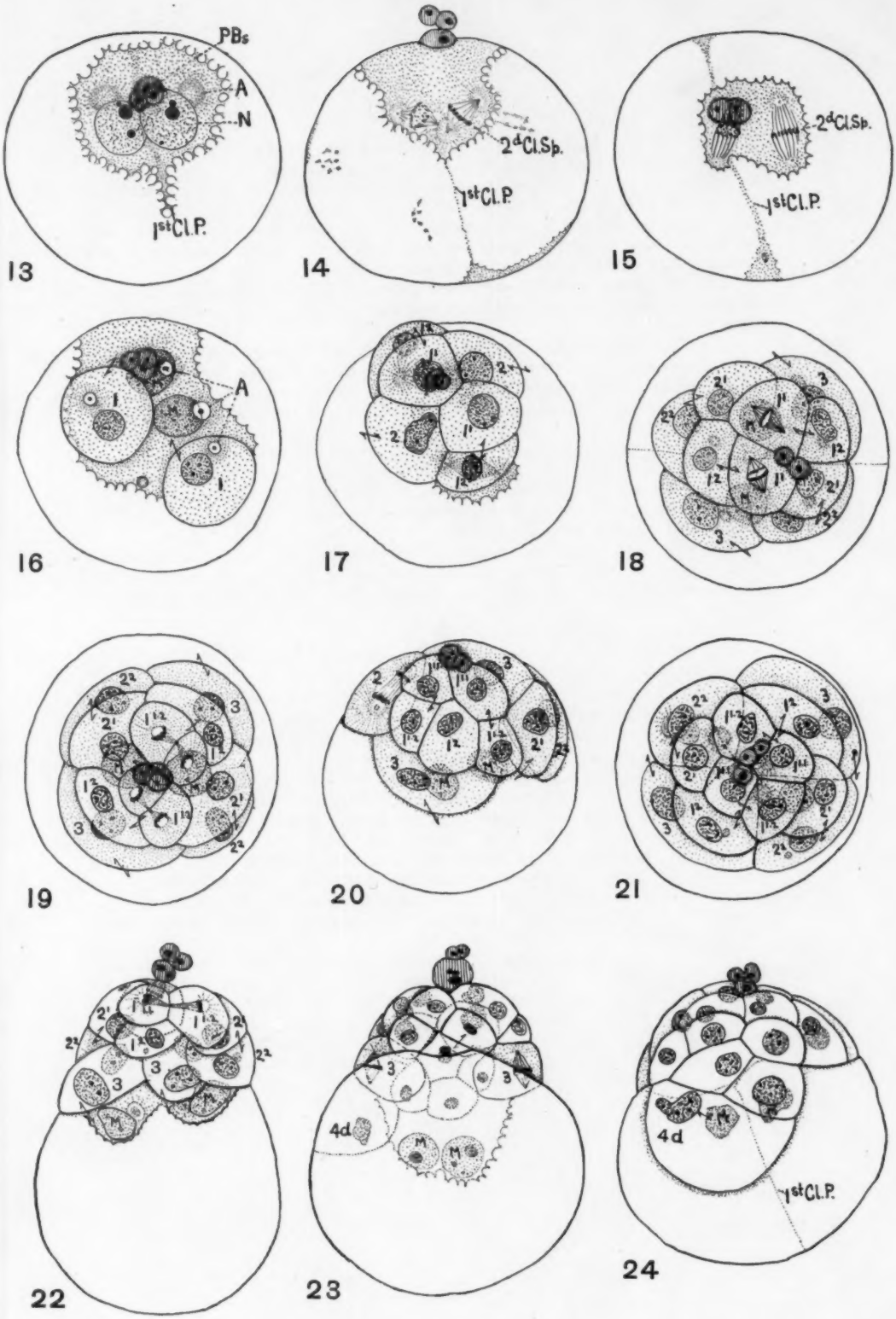

PBs
A
N
1st Cl.P.
13
2d Cl.Sp.
1st Cl.P.
14
2d Cl.Sp.
1st Cl.P.
15
A
16
17
18
19
20
21
4d
22
4d
23
4d
1st Cl.P.
24

Plate III

Figs. 25–27. Eggs in which the first cleavage spindle was turned into a position similar to that of the 3d cleavage of normal eggs with the result that micromeres are formed at the animal pole, which are more or less similar in content, position and subsequent subdivisions to the first, second, and third quartettes of typical eggs, but in sets of ones, instead of fours. Such cases are relatively rare and development is not as regular nor does it go as far as when micromeres are formed in sets of two, as in Figs. 16–24 and 28–31.

Fig. 25. Giant polar bodies; first cleavage spindle oblique to egg axis, which is marked by polar bodies, polar lobe and axial path; − 1° for 8 hrs., room temp. 14 hrs.

Fig. 26. Atypical first cleavage giving rise to a large micromere of the 1st set at the animal pole, containing a large nucleus of irregular form; two large karyomeres in the macromere have not united; − 1° for 6 hrs., room temp. 16 hrs.

Fig. 27. The first cleavage spindle was nearly vertical and cut off a micromere like one formed typically at the third cleavage except for size. This first micromere has divided unequally into a large central (1^1) and a smaller outer "turret" cell (1^2) as shown by the spindle remnant connecting the two daughter nuclei. A second micromere has separated from the single macromere, as in typical cleavage, but all the chromosomes are in the second micromere and only a centrosphere is left in the macromere; − 1° for 6 hrs., room temp. 16 hrs.

Fig. 28. Egg with one macromere containing two connected spindles, which are in process of giving off the third set of micromeres; there are six micromeres, two of which belong to the second set (2) and are in contact and not on opposite sides of the egg, as in Fig. 17, and four belong to the first set (1^1, 1^2); − 1° for 6 hrs., room temp. 16 hrs.

Fig. 29. Single macromere with two nuclei shown in dotted outline, and 12 micromeres, which were formed in sets of twos, as in Fig. 19; there are 6 of the first set, 4 of the second and 2 of the third; same slide as preceding.

Fig. 30. Egg similar to the preceding with two nuclei in a single macromere and 14 micromeres, some of which cannot be identified with certainty; − 1° for 8 hrs., room temp. 14 hrs.

Figs. 31–36. Eggs in which the typical first and second cleavages were suppressed and in which the micromeres cannot be identified with certainty.

Fig. 31. Side view of egg with one macromere containing two nuclei and with 10 micromeres; − 1° for 6 hrs., room temp. 16 hrs.

Fig. 32. Side view of egg similar to the preceding but in which one of the two nuclei (*M*) in the single macromere is abnormal in shape and has given rise to micromeres with abnormal nuclei; − 1° for 8 hrs., room temp. 14 hrs.

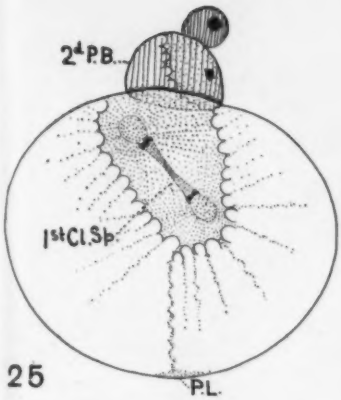
Fig. 33. One macromere containing 8 karyomeres; 16 micromeres each with one nucleus; − 1° for 8 hrs., room temp. 14 hrs.

Fig. 34. Side view of egg with two nuclei (*M*) in the single macromere and 14 micromeres, two of which are dividing; − 1° for 6 hrs., room temp. 16 hrs.

Fig. 35. Side view of egg similar to Fig. 33, and from the same slide, in which there are 8 nuclei or karyomeres in the single macromere and 16 micromeres.

Fig. 36. Egg in which the first cleavage was nearly equatorial as in Figs. 37–42 and in which the upper cell has given rise to 32 micromeres; the lower cell contains a large number of karyomeres; − 1° for 6 hrs., room temp. 23 hrs.

PLATE III



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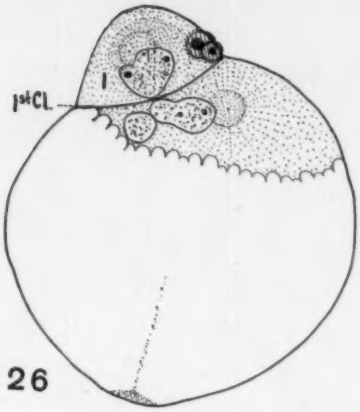

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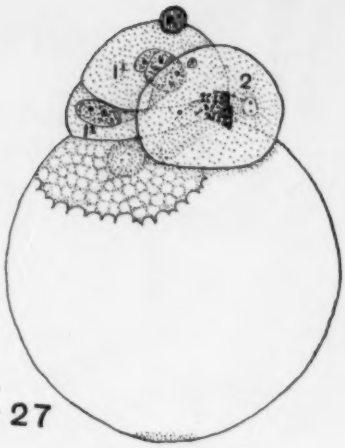

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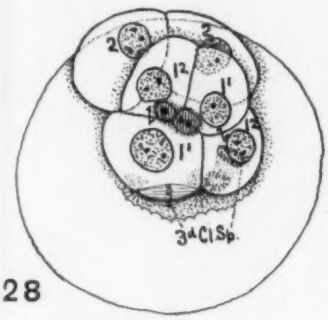

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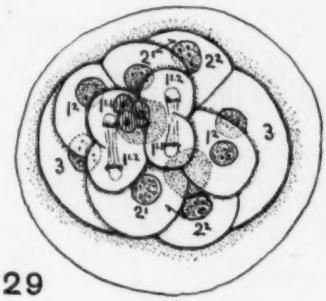
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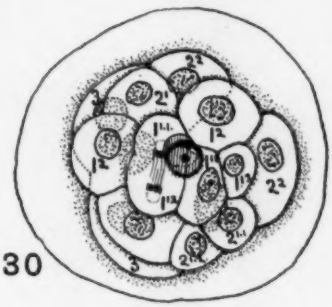
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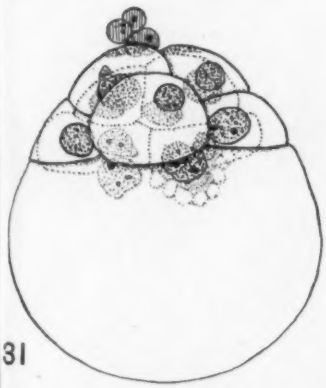
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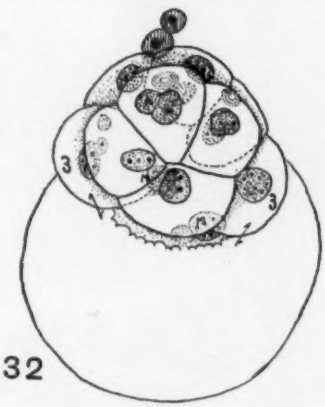
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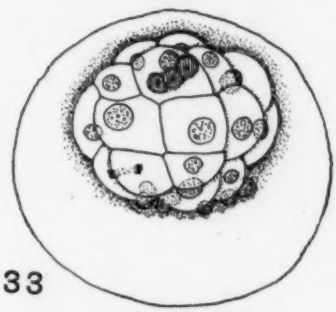
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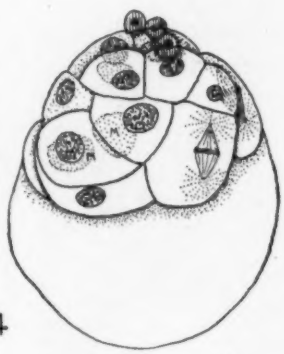
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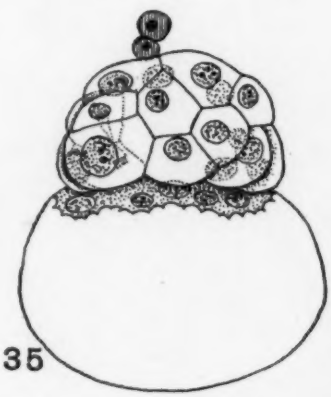
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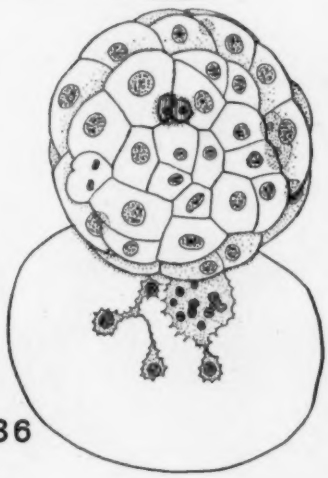
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PLATE IV

FIGS. 37–44. Eggs in which the first cleavage spindle was nearly vertical in the egg axis and the resulting cleavage furrow is nearly equatorial; the animal pole is marked by the polar bodies and the vegetative pole by the polar lobe or axial path of cytoplasm. In Fig. 37 the division wall passes through the middle of the telophase spindle; in the later stages shown in Figs. 38–43 the division appears to pass through the lower end of the spindle, but this is due to later movements of cytoplasm and nuclei toward the animal pole in both daughter cells. In Fig. 43 the upper cell is lobulated on its lower side, the indentation lying in the direction of a meridional furrow; this probably indicates an attempt on the part of the cytoplasm to divide in the normal manner. All from the same experiment, viz. − 1° for 6 hrs., then room temp. of 22° for 4 hrs.

FIG. 45. Egg in which both first and second cleavages are transverse to the egg axis; − 1° 6 hrs., then room temp. 16 hrs. The further development of such eggs is shown in Figs. 84–88.

PLATE IV

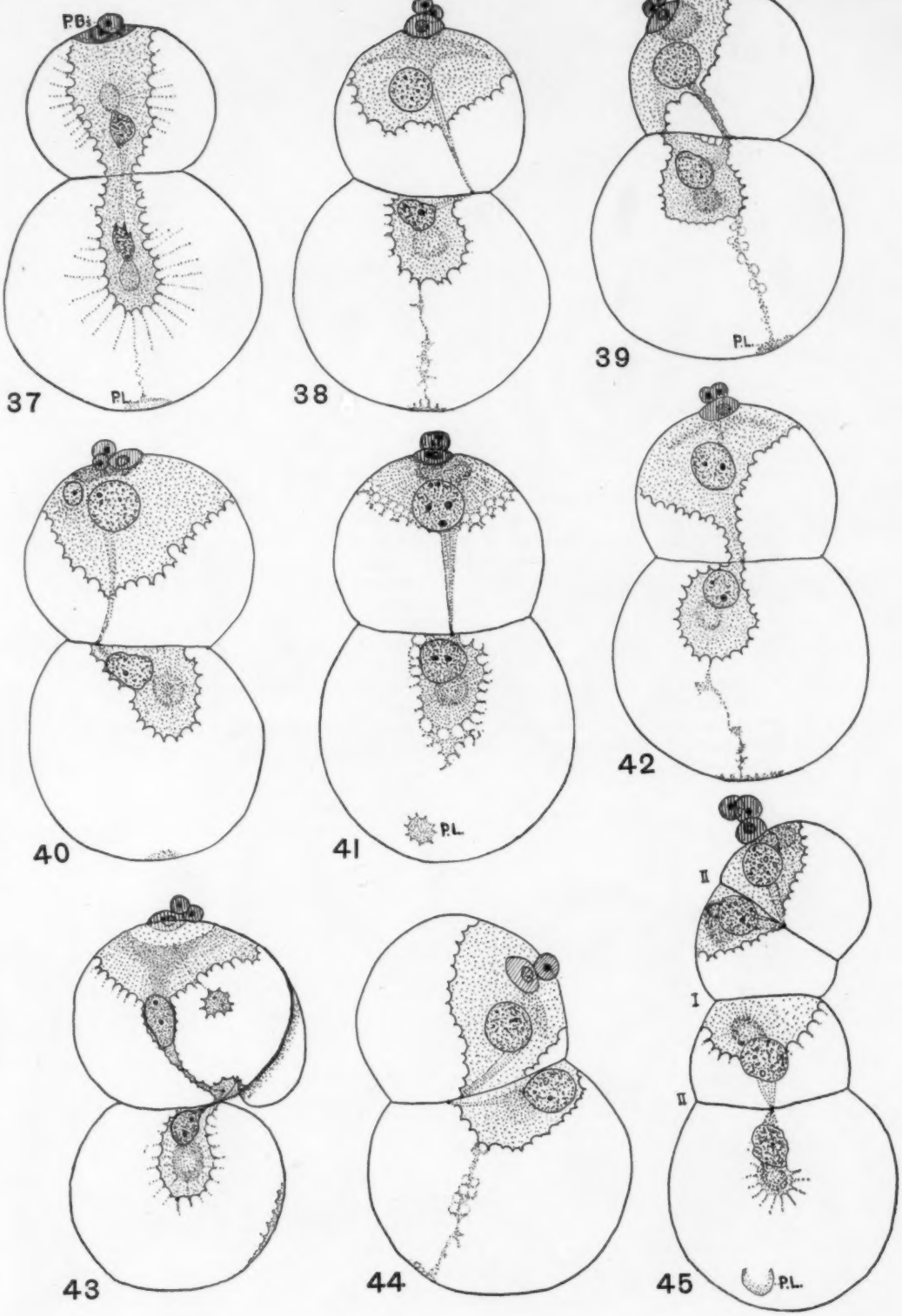


PLATE V

FIGS. 46–55. Two-cell stages subjected to − 1° to 2° C. for from 6 to 23 hrs. and then fixed at once. All show more or less clotting of cytoplasm and many division stages show dislocation of mitotic figures and cytoplasm, caused by the suppression of normal movements of the cytoplasm.

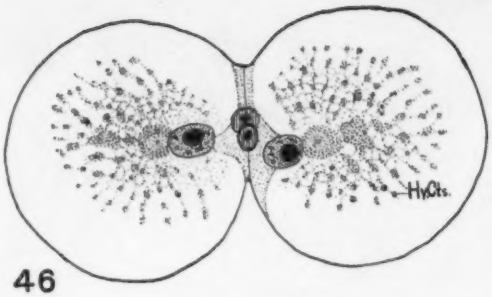
FIGS. 46–48. Marked clotting of hyaloplasm (*Hy.Cts.*) in more or less radial lines; -- 1° for 8 hrs., then fixed at once.

FIG. 49. Enormous nuclei and nucleoli with daughter centrospheres on opposite sides of nuclei; + 2° for 23 hrs., fixed at once.

FIGS. 50–51. Nuclear membrane dissolved but contents still visible with chromosome plates and centrospheres in more or less abnormal positions within the nuclear areas; + 2° for 6 hrs., fixed at once.

FIGS. 52–55. Chromosomes, centrospheres and cytoplasm in abnormal positions; cytoplasm especially abundant in cleavage furrow; + 2° for 6 hrs., fixed at once.

PLATE V



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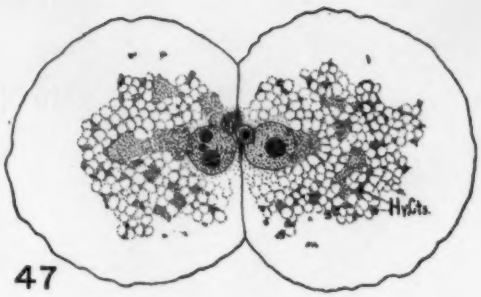

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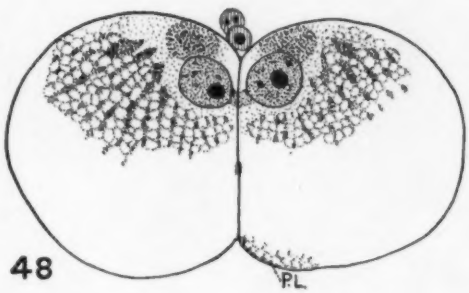

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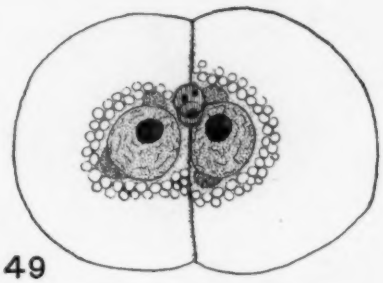

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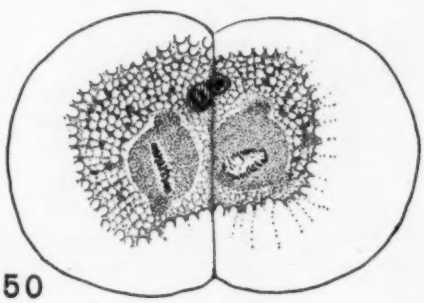

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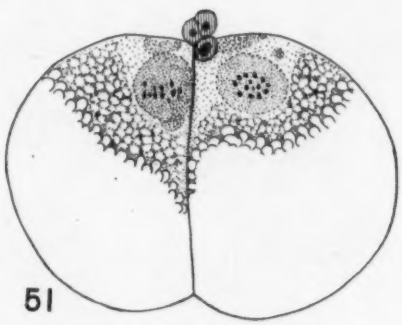

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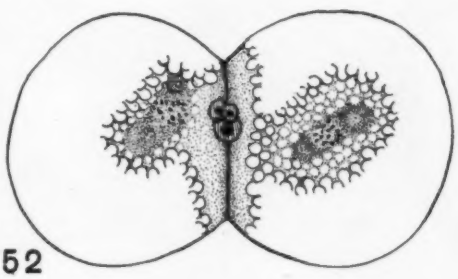

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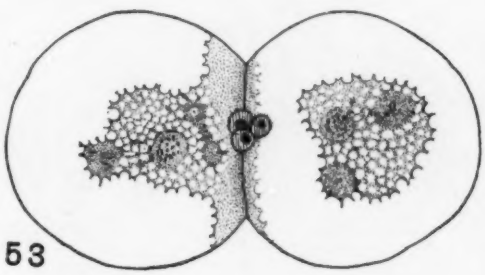

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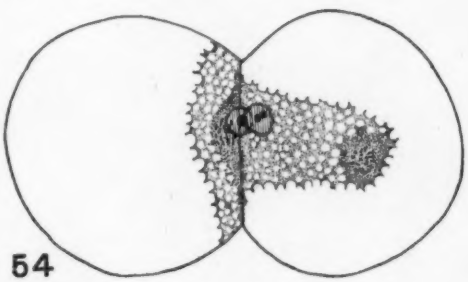

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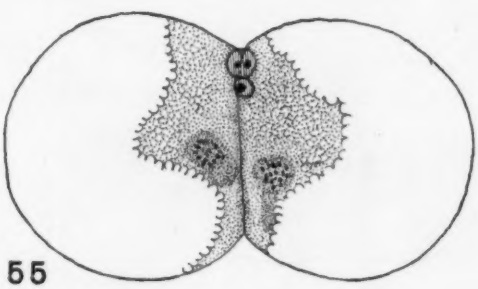


55

PLATE VI

FIGS. 56–63. Two-cell stages with mitotic figures in abnormal positions.

FIGS. 56, 57. Eggs kept at − 1° for 8 hrs. and then fixed at once.

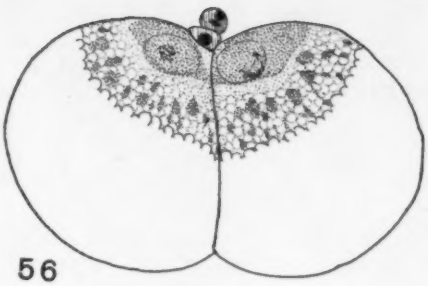
FIGS. 58, 60–63. Eggs kept at − 1° for 6 hrs., room temp. 1 hr.

FIG. 59. Egg kept at − 1° for 6 hrs., room temp. 14 hrs.

FIG. 64. Egg kept at + 2° for 23 hrs., then fixed at once. Second cleavage furrows were suppressed but nuclear division was completed and daughter nuclei and nucleoli have grown to a large size while in the cold. In the right blastomere the position of the spindle was abnormal.

FIG. 65. Two-cell stage kept at − 1° for 8 hrs., room temp. 14 hrs. The second cleavage spindles were at right angles to their normal positions as in Fig. 61. Cytoplasm is most abundant in the daughter cells to the right, possibly owing to the action of gravity.

PLATE VI



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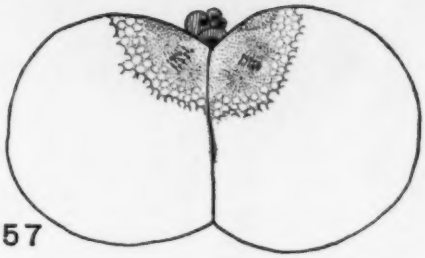

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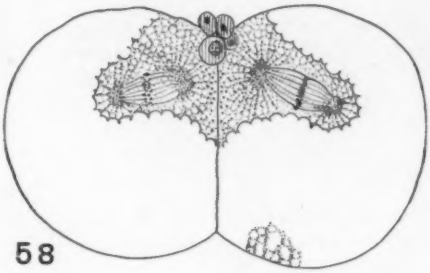

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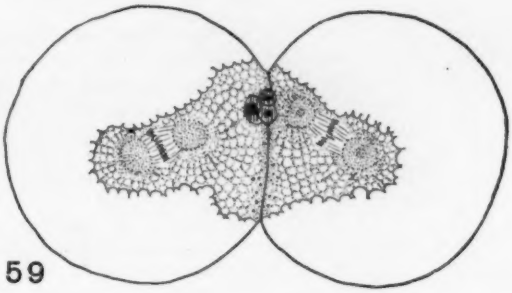

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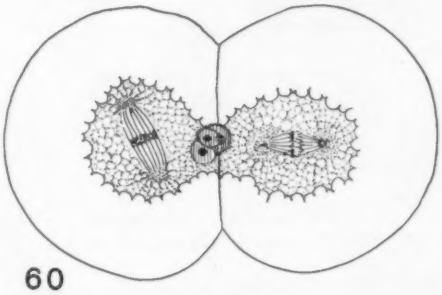

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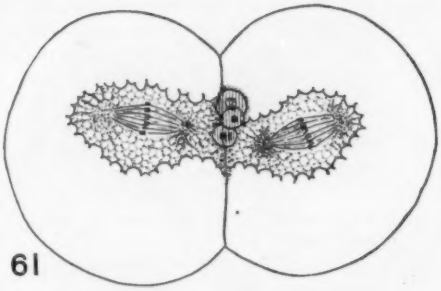

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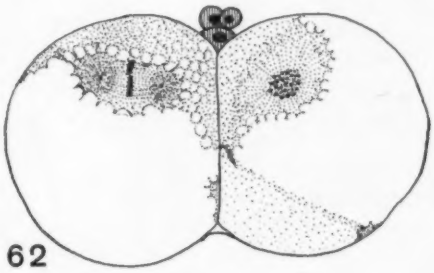

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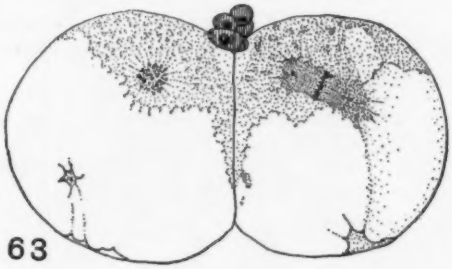

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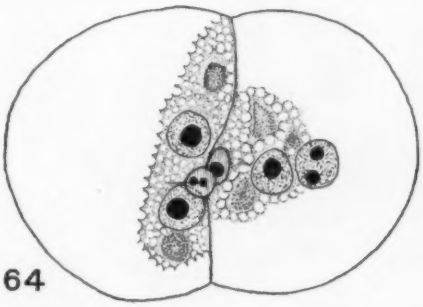

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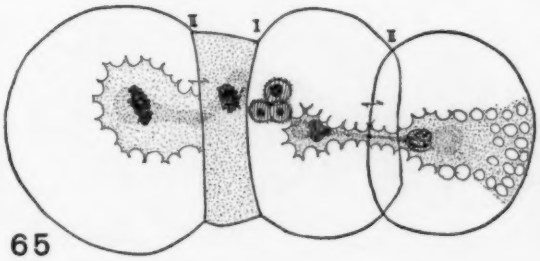


65

Plate VII

Figs. 66–75. Eggs subjected to cold in the 2-cell stage after the first cleavage was completed. The typical second cleavage was entirely omitted and there are consequently but two macromeres, each of which gives rise to micromeres that are entirely typical in contents, positions and sub-divisions, but since they are only half as numerous as in typical eggs they are twice as large.

Fig. 66. Two-cell stage kept at 0° for 4½ hrs., room temp. 24 hrs. The first set of micromeres (1) were formed by a dexiotropic direction as in the typical third cleavage.

Fig. 67. Side view of egg like Fig. 66, granular sphere substance at apical surface of first set of micromeres; material of the polar lobe shows at the lower pole of the left (posterior) macromere; − 1° for 4½ hrs., room temp. 14 hrs.

Fig. 68. Second set of micromeres forming in læotropic direction, as in the typical fourth cleavage; coarse granules of sphere substance at the upper ends of the spindles as in normal eggs; 0° for 4½ hrs., room temp. 24 hrs.

Fig. 69. First micromeres subdividing in læotropic direction; nuclei and spheres in second micromeres and macromeres perfectly normal in position; − 1° 6 hrs., room temp. 14 hrs.

Fig. 70. Side view of egg similar to Fig. 69, one of first micromeres subdividing in typical manner; polar lobe at lower side of left (posterior) macromere; same experiment as Fig. 69.

Fig. 71. Side view of stage like Fig. 74, in which the first and second micromeres have subdivided and the third has formed; same experiment as preceding.

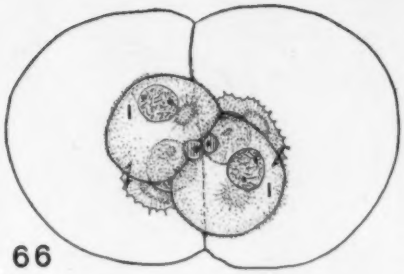
Fig. 72. Apical view of stage a little older than Fig. 69 in which the first micromeres have subdivided into a large apical (1^1) and a small peripheral "turret" cell (1^2); 0° for 4½ hrs., room temp. 24 hrs.

Fig. 73. The third micromeres have been cut off in a dexiotropic direction, while the second are subdividing, all in typical manner; − 1° for 6 hrs., room temp. 14 hrs.

Fig. 74. All three sets of micromeres present in typical positions and shapes, but double in size and half the number of those in typical eggs; − 1° for 6 hrs., room temp. 22 hrs.

Fig. 75. Second subdivision of first micromeres in dexiotropic direction, other micromeres as in Fig. 74; − 1° for 6 hrs., room temp. 16 hrs.

PLATE VII



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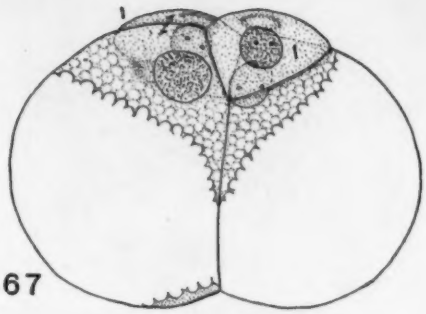

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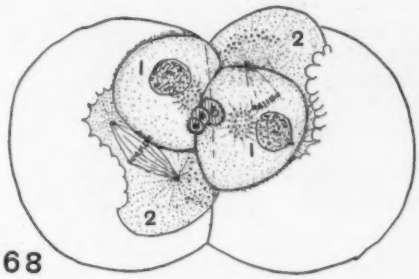

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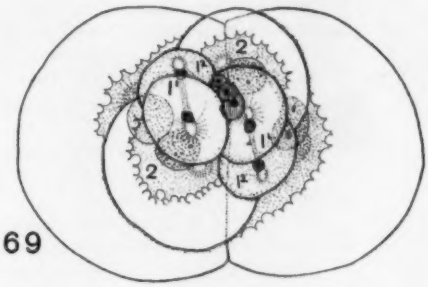

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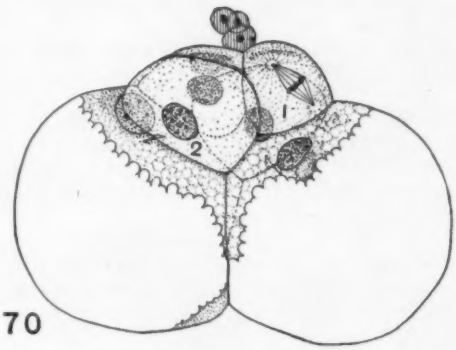

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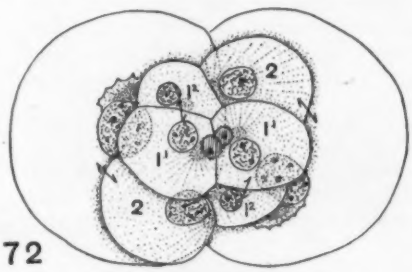

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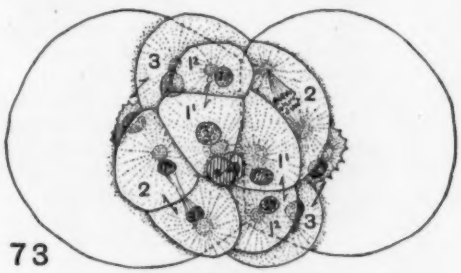

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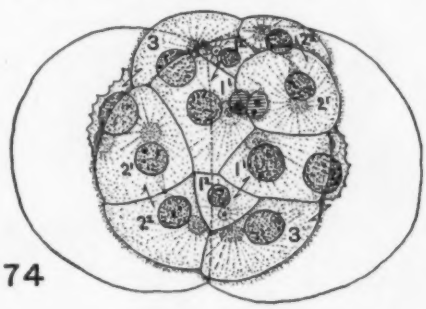

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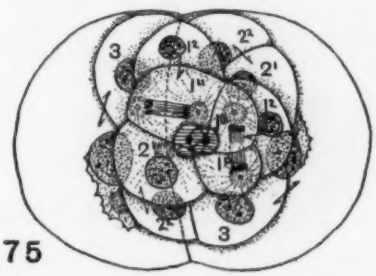

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PLATE VIII

FIGS. 76–79. Eggs subjected to cold in the 2-cell stage in which the typical second cleavage was omitted, but in which the micromeres are not entirely typical in position or order of formation and subdivision.

FIG. 76. First, second and third micromeres are typical (as in Fig. 74) but a large fourth micromere (4*d*) has separated from the right macromere in a *dexiotropic* direction, and *before* the second subdivision of the apical cells; − 1° for 6 hrs., room temp. 23 hrs.

FIG. 77. Egg with three large nuclei in the right macromere and with 18 micromeres which cannot be individually identified; − 1° for 6 hrs., room temp. 23 hrs.

FIG. 78. Side view of stage like Fig. 77 with micromeres crowded down into the furrow between the macromeres; same preparation as Fig. 77.

FIG. 79. First two macromeres widely separated with numerous micromeres between them; 1° for 24 hrs., room temp. 10 hrs.

FIG. 80. Egg with 5 macromeres and 22 micromeres; each of the macromeres has just divided, forming 5 micromeres of the third set (3); 1° for 24 hrs., room temp. 10 hrs.

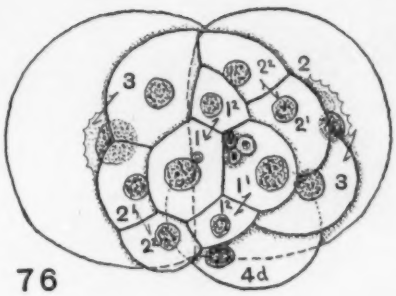
FIG. 81. Egg with 3 macromeres, each of which has given off three micromeres; the micromeres of the first set have subdivided once into apicals (1^1) and small "turrets" (1^2), two of the second set are subdividing and the third set is just coming off from each of the macromeres; absolutely typical except that all are in threes instead of fours; − 1° for 6 hrs., room temp. 23 hrs.

FIGS. 82–89. Very abnormal cleavages with four or more macromeres which are abnormal in positions and subdivisions owing to dislocation of mitotic figures and cleavage planes. In all these the first or second cleavage plane was transverse or oblique to the egg axis, which is marked in all cases by the polar bodies, and in Figs. 87 and 89 also by the polar lobe.

FIG. 82. One of the first two cleavages, probably the second (II), was transverse to the egg axis. Cytoplasm and mitotic figures lie on the animal pole side of each cell. Coarse granules over the upper ends of the spindles in the lower cells are like the granules of sphere substance in the second set of micromeres of normal eggs (see also Fig. 68).

FIG. 83. Later stage of an egg similar to Fig. 82. Only two cells have their cytoplasm and mitotic figures at the animal pole; in the other cells cytoplasm and nuclei are centered on an area about 90° from the polar bodies; probably the first cleavage (I) was approximately normal, the second (II) transverse to the chief axis with most of the cytoplasm below this cleavage plane; − 1° for 6 hrs., room temp. 23 hrs.

PLATE VIII



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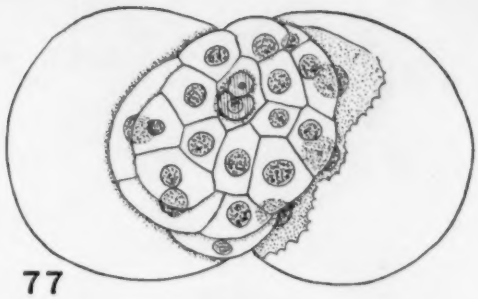
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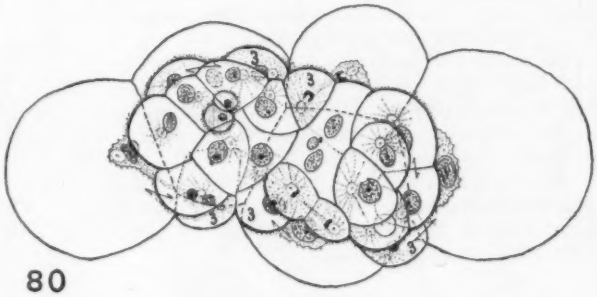

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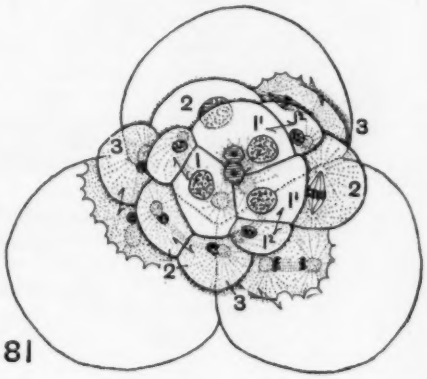

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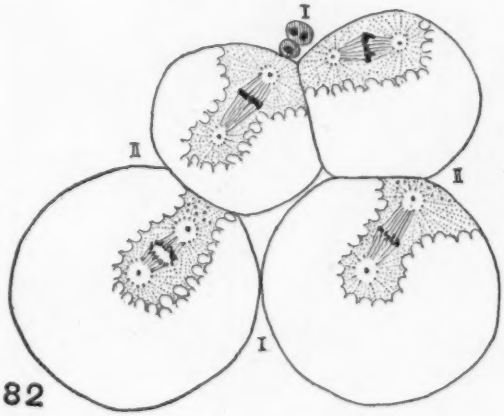

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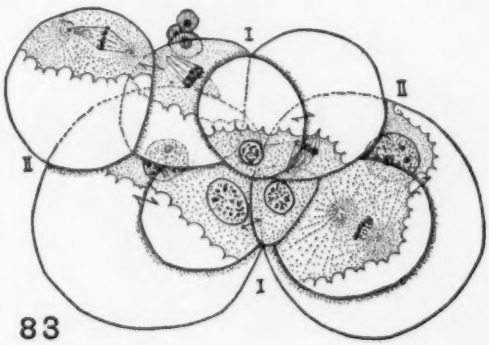

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PLATE IX

FIG. 84. Abnormal 4-cell stage in which both first and second cleavage planes were oblique to the egg axis, and in which polarity is partially reversed in the right cell, possibly owing to rotation of this cell after its formation; − 1° for 6 hrs., room temp. 22 hrs.

FIG. 85. Abnormal 8-cell stage in which the first cleavage (I) was transverse to the egg axis and the lower half then gave off two or three micromeres at its upper side while the upper half divided irregularly into four or five cells; − 1° for 6 hrs., room temp. 23 hrs.

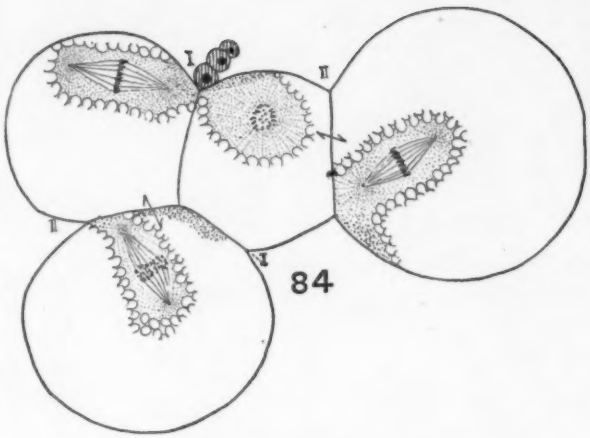
FIG. 86. Very abnormal 8-cell stage in which the first (I) cleavage was approximately normal while the second (II) and third (III) cleavages were abnormal in position; − 1° 6 hrs., room temp. 23 hrs.

FIG. 87. Seven cells (one with two nuclei, as shown by the bond) in which first, second and third cleavages were abnormal in position; the egg axis is marked by polar bodies and polar lobe substance and both first and second cleavages were transverse or oblique to this axis; − 1° for 6 hrs., room temp. 23 hrs.

FIG. 88. Eight-cell stage in which the first cleavage (I) was nearly in the egg axis but the second (II) and third (III) were out of position (the third cleavages are marked by bonds). In spite of abnormal cleavage the cytoplasm and nuclei are on the animal pole side of each cell, showing that the intrinsic polarity of each cell is but little changed; − 1° for 6 hrs., room temp. 23 hrs.

FIG. 89. Eight-cell stage in which the first cleavage (I) was nearly in the egg axis but the second (II) was transverse to it. Each of the four macromeres so formed has given off a large micromere on its upper side and two of these micromeres are dividing; same preparation as the preceding.

PLATE IX



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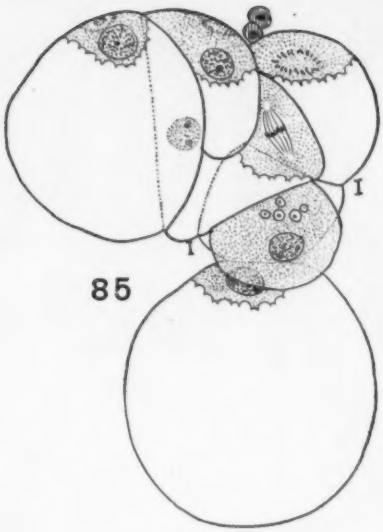

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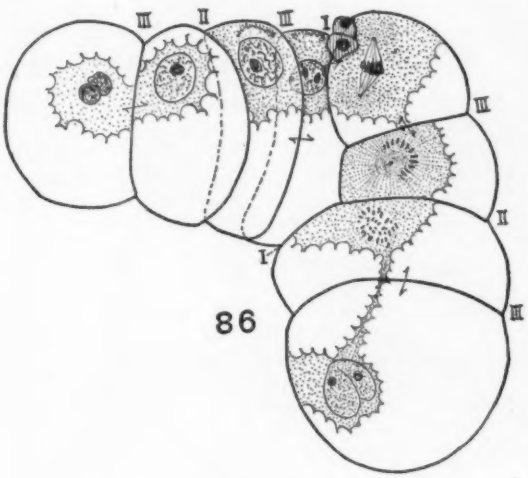

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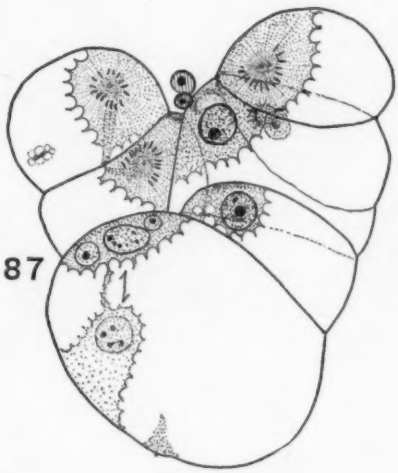

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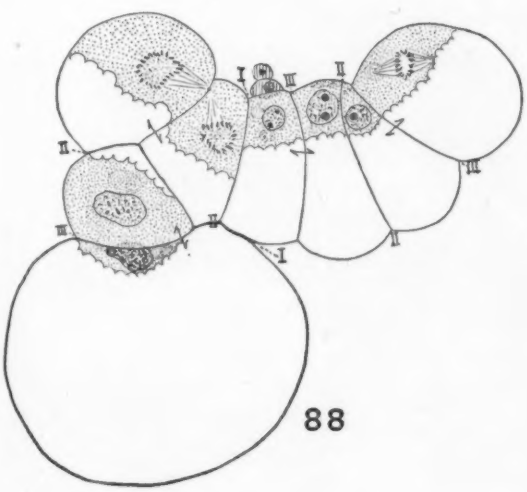

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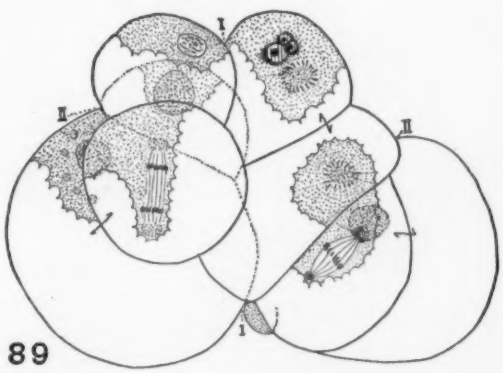

89

THE AMERICAN SPECIES OF THE GENUS STENOSTOMUM

JOHN W. NUTTYCOMBE AND AUBREY J. WATERS

Department of Zoölogy, University of Georgia

(Communicated by Edwin G. Conklin)

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ABSTRACT

Various species of *Stenostomum* have been widely used for experimental purposes but the results of the work have been confused because of the uncertainty of specific identication. A review of the literature shows 30 species previously described from America, 18 of which appear to be of unquestionable validity. Descriptions and plates (with specific diagnoses and a key) are furnished for these 18 and 4 new species. Brief notes on the biology of the genus give a working knowledge of collection and culture methods.

INTRODUCTION[1, 2, 3]

SPECIES of *Stenostomum* exhibit many characteristics which fit them well for various types of experimental work. They

[1] To Dr. William A. Kepner, University of Virginia, for his criticism of the manuscript and the use of his library on rhabdocoels; to Dr. Alexander Vyssotsky, University of Virginia, for translating papers from the Russian and to Mrs. J. W. Nuttycombe for invaluable assistance with the literature we wish to express our gratitude.

[2] A substantial part of this work was done at the Mountain Lake Biological Station, of the University of Virginia during the summer of 1936 when the senior author held a research fellowship at the Station.

[3] The senior author alone is responsible for the determination of species validity and for most of the material in the history of the genus.

culture easily, reproduce rapidly, regenerate extensively and are so simply constructed that internal changes may be readily observed. Several species have been widely used for studies of varied types. Child (1901, 1902*a*, *b* and *c*, 1903), Van Cleave (1929, 1934), Sonneborn (1930), Carter (1933), Kepner and Yoe (1933), Nuttycombe (1932*a*) and others have used members of the genus for studies of axial gradients, normal reconstitution, reconstitution following x-radiation, genetics, the action of vital stains, phagocytosis, and for other experimental work. It appears to us that several species of the genus are ideally adapted to studies in population problems.

The results of past work have been complicated several times by the fact that accurate specific identifications were not made. Van Cleave (1929) sharply focused attention on the effect which incorrect specific identification might have upon experimental results. He found that the reconstitutional behavior of *S. tenuicaudatum* [1] was like that described by Child (1903) for *S. leucops* and that the species which he (Van Cleave) identified as *S. leucops* had a very different reconstitutional pattern. Since that time it has become apparent that many of the specific identifications of the past have been faulty.

The primary purpose of this paper is to describe the present American species of the genus so that accurate diagnosis will be possible to future workers. It is not the purpose of this paper to serve as an exhaustive source of information concerning the genus. We have however attempted to locate all the literature on the genus in order that we might have a fairly complete history of the various species and in order that we might select the most important papers for citation here. We believe we have been able to obtain all the papers of real significance. A few others we have been unable to locate and know only from abstracts. In our attempt to limit the length of this paper we have found it necessary to reduce all sections

[1] We are using the specific name *tenuicaudatum* as preferable to the original name *tenuicauda* (Graff, 1911) which has been universally used until now.

to a minimum and in certain sections, notably those on the history of the genus and the general biology of the genus, the space allotted is hardly consistent with the importance of the topics. Even in these abbreviated sections we have tried to include sufficient material and bibliography to serve as a groundwork for one who wishes to make a detailed study.

Brief History of the Genus Stenostomum

Dugès (1828) described *Derostoma leucops* which was later removed to a new genus, *Stenostoma*, by O. Schmidt (1848). Schmidt at this time added to the new genus a second species, *S. unicolor*, and in 1852 he added a third species, *S. torneense*. Schmidt (1858) placed the genus in the family Microstomeen and Schmarda (1859) placed the genus in a rhabdocoel Suborder Pharyngea, Family Derostoma. Diesing (1861) placed Stenostominea in a group, Arhynchocoelo proctucha, along with *Microstomum* and others. Vejdovsky (1882) moved the genus into his new family Stenostomidae and included his two new species, *S. fasciatum* and *S. ignavum*. Graff (1882) placed the genus in the family Microstomidae and listed the following species: *S. gracile*,[1] *S. lemnae*,[2] *S. quaternum*,[3] *S. coluber*,[4] *S. sieboldii*,[5] *S. binum*,[6] *S. unicolor*, *S. ignavum*,[1] *S. leucops*, and *S. fasciatum*.[7] Graff (1913) mentions in his bibliography for *Catenula lemnae* a *Stenostomum monocoelis* described by Schmidt (1878). The genus continued for some time in and out of the families Microstomidae and Stenostomidae. Keller (1894) carried the genus in Vejdovsky's Stenostomidae whereas Plotnikow (1905 *a*, *b*) located the genus in the Microstomidae. Graff (1905) set up the family Catenulidae (named for the first genus, *Catenula*, in the group) which included the genus *Stenostomum*. He pointed out in this paper that the original generic name, *Stenostoma*, should be discarded for

[1] Described as *Anortha gracilis* by Leidy, 1851.
[2] Described as *Catenula lemnae* by Dugès, 1832.
[3] Described as *Catenula quaternum* by Schmarda, 1859.
[4] Leydig, 1854.
[5] Graff, 1878.
[6] Described as *Catenula bina* by Schmarda, 1859.
[7] Vejdovsky, 1879.

Stenostomum because of the likelihood of confusion of the former name with one used in 1810 in the Coleoptera. Graff carried the family Catenulidae into Bronn's Klassen und Ordnungen des Tierreichs (1904–1908).

Since the family Catenulidae was defined there has been little tendency to place the genus *Stenostomum* in any other family. Luther (1907) discussed the reasons for reorganization of the family Catenulidae. In the family Luther placed the following genera: *Catenula, Lophorhynchus, Stenostomum* and *Rhynchoscolex*. Luther incidentally included sufficient historical material on the genus *Stenostomum* to serve as an excellent background for a study of the genus from this viewpoint. Sekera (1924) discussed the taxonomic relationships of *Catenula* and *Stenostomum* within the family Catenulidae and Reisinger (1924) sharply defined the sub-order Notandropora and discussed the history and systematic position of the Catenulidae.

Graff (1913) placed the genus in the family Catenulidae which he placed in the section Hysterophora and listed, with descriptions, 15 species of *Stenostomum* which he considered to be valid. His species were as follows: *S. leucops, S. tenuicaudatum,*[1] *S. caudatum,*[2] *S. grande,*[3] *S. gilvum,*[4] *S. middendorffii,*[5] *S. coluber, S. unicolor, S. ignavum, S. fasciatum, S. agile,*[6] *S. langi,*[7] *S. stuhlmanni,*[4] *S. bicaudatum,*[8] and *S. sieboldii*. It will be noted that 4 of the species in Graff's 1882 list are missing from the 1913 list, namely: *S. lemnae, S. quaternum, S. gracile* and *S. binum*. These species in 1913 fell either into the genus *Catenula*, as then defined, as species or into synonymy within it. *S. torneense* and *S. neoboracense*[9] were listed by Graff (1913) as synonyms for *S. leucops* and *S.*

[1] Graff, 1911.
[2] Markow, 1904.
[3] Child, 1902 *a*.
[4] Bohmig, 1897.
[5] Braun, 1885.
[6] Silliman, 1885.
[7] Keller, 1894.
[8] Kennel, 1889.
[9] Girard, 1893.

turgidum [1] was removed to the genus *Fuhrmannia*. *S. hystrix* [2] is carried by Graff (1913) in a group designated as "Catenulidarum species dubiae" but according to Hofsten (1912) Keller agreed that his (Keller's) *S. hystrix* was a young annelid. Beklemichev in 1920 added *S. coluber* to the synonymy of *Rhynchoscolex*. Certainly *S. coluber* appears to belong elsewhere than in the genus *Stenostomum*. Meixner (1924) quite rightly removed *S. sieboldii* from the Catenulidae. Sekera (1903*a*) believed *S. ignavum* to be a spring adapted *S. leucops* and Hofsten (1912) questioned the validity of *S. langi*. *S. langi* seems to us however a valid species which was questioned by Hofsten because of the inaccurate descriptions of some details by Keller in 1894. Markow's description of *S. caudatum* seems to us insufficient to establish a species on a sound basis. The same thing holds good for Böhmig's *S. gilvum* and Böhmig's *S. stuhlmannii*. We have in this paper treated *S. leucops* and *S. agile* in a section devoted to species which are, to us, not very sharply defined.

Our chief interest in the European species has been in connection with the establishment of the validity of the American species and not in any sense from a desire to clarify the species of Europe or elsewhere. We would however like very much to see someone in Europe redescribe the European species and set them in order.

Since 1913 there have appeared in the literature 26 new specific names: 3 from Europe, 1 from China and 22 from the United States. From Europe Nasonov (1924) described *S. tauricum* and *S. hemisphaericum* and Gieysztor (1931) described *S. arevaloi* which Nuttycombe (1932*b*) later erroneously described as a new species, *S. rhachiocaudatum*, from the United States. Tseng-Jui Tu (1934) indecisively described *S. tsinghuaensis* from China. The 22 described species from the United States are discussed in the sections on valid and questionable species.

Including the 4 new species described in this paper there appear to be 22 sharply defined species in the United States.

[1] Zacharias, 1903.
[2] Keller, 1895.

In addition *S. agile*, *S. incaudatum* and possibly others, which we have included in our questionable groups, may be perfectly valid species badly in need of redescription but we have been unable to find specimens for study.

Taxonomic Position and Description of the Genus

The latest extensive work of the Turbellaria is that of Bresslau (1933) in Handbuch der Zoologie in which the genus *Stenostomum* is placed in the family Catenulidae, sub-order Notandropora, order Rhabdocoelida. This position of the genus seems perfectly sound as the family Catenulidae is now defined.

Graff (1913) placed in the genus *Stenostomum* those Catenulidae in which the statocyst and preoral furrow are absent and which usually have paired ciliated pits and colorless light refracting bodies. Since that time several species have been described which lack light refracting bodies.

The body is roughly cylindrical but is, as a rule, somewhat flattened ventrally. In most species the body tapers at both the anterior and posterior extremities. The posterior end is generally the more pointed. There is frequently a more or less distinct head region. See Fig. 1 for schematic optical sections of *Stenostomum*.

The epidermis is ciliated and in most species its cells bear in their distal ends rod-shaped rhabdites.

Typically the head bears on each side a ciliated pit which is embedded in, or very closely associated with, the anterior lobe of the cerebral ganglion on the respective side. The "brain" or cerebral ganglia (Fig. 2) is usually large and prominent and is composed of paired anterior and posterior lobes. The anterior lobes are in some species larger than the posterior lobes. The mesial margins of the anterior lobes are usually deeply dentate. The right and left ganglionic masses are connected by a substantial transverse commissure. From each ganglionic mass 2 nerves pass posteriorly. Light refracting bodies may or may not be associated with the cerebral ganglia. In some species there is a single pair of such bodies; in others

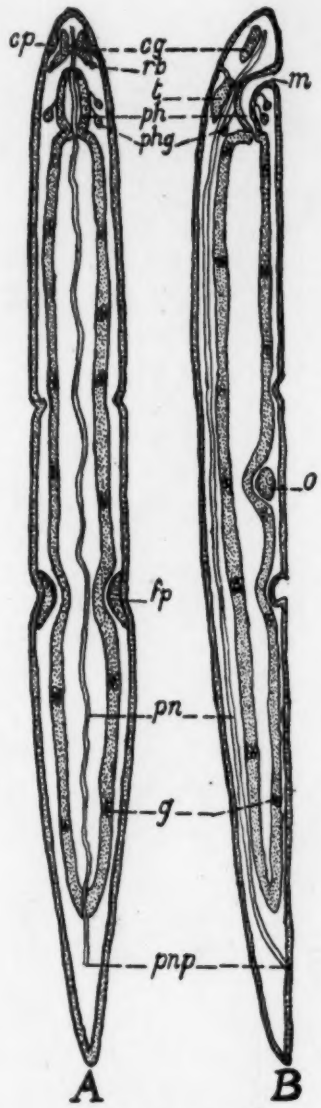

FIG. 1. *A*. Diagrammatic, optical view of *Stenostomum* from the dorsal side. *B*. Lateral view of same. *cg*, cerebral ganglion; *cp*, ciliated pit, *fp*, developing fission plane; *g*, enteric "gland" cells; *m*, mouth; *o*, ovary; *ph*, pharynx; *phg*, pharyngeal glands; *pn*, protonephridium; *pnp*, protonephridiopore; *rb*, light refracting body; *t*, testis.

there may be more than one pair. The association between these bodies and the ganglia is variable in the genus but in most species they are associated with the posterior ganglionic lobes.

The mouth is located mesio-ventrally near the anterior end of the zooid. It is equipped with a sphincter (and in some cases special musculature) and opens into a simple (generally), tubular pharynx the walls of which are usually well-muscled. In many cases the outer wall of the pharynx bears unicellular glands. There may be also an elaborate radial musculature

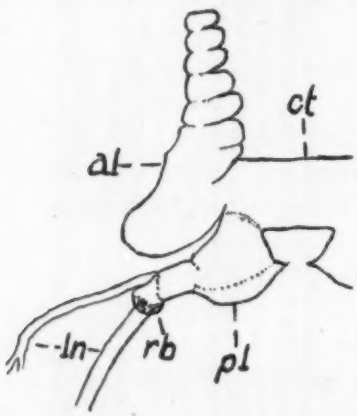

FIG. 2. Dorsal view of left half of brain of *Stenostomum tenuicaudatum*. *al*, anterior lobe; *ct*, transverse commisure; *ln*, lateral nerves; *pl*, posterior lobe; *rb*, light refracting body.

from the pharynx to the body wall. The pharynx opens posteriorly through a sphincter into the straight intestinal tube.

The pseudocoel between the outer body wall, brain, pharynx, intestine, and other body parts, is traversed by radial muscle fibers and contains free cells of various shapes and sizes. The interstices are filled with a colorless plasma of undetermined consistency.

The protonephridium lies in the pseudocoel and is composed of two stems. From the single posterior nephridiopore the larger tube passes anteriorly above the enteron and

pharynx (except in *S. ventronephrium*) and then passes ventrally beneath the transverse cerebral commissure. Just in front of the commissure it bends upon itself to form the smaller capillary tube which passes posteriorly along the same general path as that followed by the larger tube. Reisinger (1922) and Kepner and Yoe (1933) have considered the structural and functional differences between the larger and capillary tubes of the protonephridium.

Reproduction is usually asexual by transverse fission. Periodic sexual reproduction has been observed in several species. The male gonad occurs in the antero-dorsal part of the body; the ovaries in the pseudocoel near the middle of the zooid. No ducts have been observed with the female gonad.

Method of Treatment of the Genus

The literature reveals that 27 species have been described from America as new. As previously mentioned, Graff (1913) placed *S. neoboracense* in the synonymy of *S. leucops* and surely Girard's (1893) description would not justify considering *S. neoboracense* as a valid species. Beklemischev (1920) removed *S. coluber* to the genus *Rhynchoscolex*. We have already in this paper removed *S. rhachiocaudatum* to the synonymy of *S. arevaloi* and, as will appear later, we are placing *S. oesophagium* in the synonymy of *S. grande*, *S. carnivorum* in the synonymy of *S. virginianum* and *S. gigerium* in the synonymy of *S. tauricum*. This leaves a total of 21 previously described American species for consideration.

Some of the descriptions of these species are so imperfect as to leave grave doubt as to the validity of the species involved and in these cases we have found it difficult, if not impossible, to separate the valid from the questionable.

Our treatment of the genus is a more or less arbitrary one but we believe it will clarify the present muddle. We have set up three groups to include all the species described from or collected in the United States. The first of these groups contains only those species (*S. bicaudatum*, *S. grande*, *S. tenuicaudatum*, *S. uronephrium*, *S. virginianum*, *S. ciliatum*,

S. membranosum, *S. simplex*, *S. mandibulatum*, *S. brevipharyngium*, *S. saliens*, *S. glandulosum*, *S. predatorium*, *S. tauricum*, *S. arevaloi*, *S. ventronephrium*, *S. pseudoacetabulum*, *S. cryptops* and the 4 new species, *S. kepneri*, *S. tuberculosum*, *S. anops* and *S. pegephilum* herein described) which we consider to be of unquestionable validity. The second group (*S. agile*, *S. incautadum*, *S. leucops*) consists of species which we think are possibly valid but which should be redescribed. We have not attempted this because of inability to obtain specimens which fit the inadequate descriptions given. In the third group we have placed those specific names (*S. speciosum*, *S. glandiferum*, *S. giganteum*) which, to us, do not have sufficient meaning to justify serious consideration.

We have included in our key only those species in the first group.

We have listed certain papers with each description and, except for *S. leucops*, the lists include practically all the literature except mere collection records. The literature on the American species, except that on *S. leucops* (much of which should fall into the synonymy of *S. tenuicaudatum*), is limited. Our bibliographic files show references to *S. leucops* in well over 100 papers but we have listed only the major descriptive papers on this species.

Another problem which we have encountered is that of varieties. We have been culturing for several years 4 varieties of *S. tenuicaudatum* and during the summer of 1936 we observed still another variety of this species at Mountain Lake, Virginia. These varieties show fairly constant minor differences such as length of free tail region, shape of anterior end, etc., which we consider of less that specific value but which are sufficiently evident to enable us to distinguish the varieties from each other. We do not believe these varieties would be easily distinguished by persons less familiar with the genus.

We have, wherever possible, redescribed or amplified the descriptions of all species which we have been able to obtain. Our descriptions are based on the study of many specimens of the species involved. We have been able to maintain cultures

of some species for several years and these have shown no morphological changes in their artificial environment. A few species do not culture easily and the descriptions are based on numerous specimens from their natural environments.

Descriptions or identifications should not be attempted from a few specimens lest stress be placed upon some variable feature such as enteric vacuoles, color of the enteron or prominence of granular enteric cells. Such features vary with the physiological state of the organism. Gross size is another feature which shows a wide range of variability; for instance, the length of *S. virginianum* ranges from 0.2 mm. to 5.0 mm. This range overlaps the ranges of practically all other species. In short, there are many features which have very little value in specific diagnoses.

We have attempted to use in our key, and stress in our descriptions, those anatomical features which are least variable and thus are of most value as specific characters. In this category we may list such features as the position and nature of the light refracting bodies, the position of the protonephridiopore, the pharyngeal structure and certain glandular bodies of the epidermis.

It seems highly desirable at this point to discuss briefly the nature of some of the characteristics which we have used in our descriptions and key for the purpose of specific diagnoses.

The light-refracting element itself (Fig. 3) may be a granule, plate, rod or may be composed of several such units of the same type. Typically a light-refracting body has associated with it a clear or finely granular vesicle which, in turn, is usually closely associated with a portion of the cerebral ganglia. In some cases where the association of the vesicle and the ganglion is not evident it may be assumed that a nerve tract passes from the vesicle to the ganglion although casual examination does not reveal such tracts.

The light-refracting bodies of different species differ as to type, number and position. Von Graff (1913) recognized three types of light-refracting bodies as follows: dish-shaped,

formed by many refractive spherules; shell-shaped, rounded vesicles with incompletely thickened refractive walls; and lens-like, lens-shaped or saddle-shaped refractive bodies. We have found some slight irregularities in the refractive bodies of such forms as *S. virginianum*, *S. glandulosum*, *S. arevaloi*, and *S. bicaudatum*. Thus, in *S. virginianum* the refractive body may appear as a flattened triangular plate or may show such indentations as to present a tri-lobed appearance (Fig. 3, *b* and *c*). Approximately similar conditions exist in *S. uronephrium*. In *S. glandulosum*, *S. arevaloi* and *S. bicaudatum* the

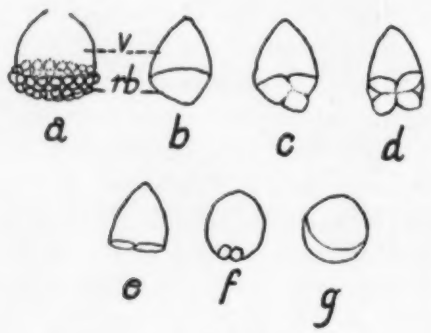

FIG. 3. Varieties of light refracting bodies. *v*, vesicle; *rb*, refracting unit; *a*, light refracting body of type 1 formed of many refractive spherules as in *Stenostomum tenuicaudatum*; *b* and *c*, light refracting bodies of type 2 as found in *Stenostomum virginianum*; *c* and *d*, light refracting bodies of type 2 as in *Stenostomum uronephrium*; *e*, light refracting body of type 2 as in *Stenostomum ciliatum*; *f*, light refracting body of type 2 as in *Stenostomum glandulosum*; *g*, light refracting body of type 2 as in *Stenostomum cryptops*.

number and positions of the refractive spherules vary somewhat. In *S. cryptops* each posterior refractive body is composed of a single unit so shaped and applied to its vesicle as to give it the appearance of a shallow dish (Fig. 3, *g*). The anterior body in this species is a spherule associated with a vesicle. According to Kepner and Carter (1931) *S. simplex* exhibits a pair of light refracting bodies each of which is a simple granule unassociated with a vesicle. The following scheme of classification of light refracting bodies seems better adapted to the American species than the scheme of von Graff.

1. Bodies each of which is composed of more than 10 granules or spherules so arranged over a part of the surface of the associated vesicle as to take the form of a shallow dish (Fig. 3, *a*).

2. Bodies each of which is composed of 1 to 5 granules or spherules, plates or rods associated with a vesicle or, of a single-unit, dish-shaped body associated with a vesicle (Fig. 3, *b, c, d, e, f, g*). This single-unit, dish-shaped body would correspond to Graff's shell-shaped type with incompletely thickened wall.

3. Single-granule bodies unassociated with a vesicle.

In the first group would fall such refractive bodies as those of *S. grande* and *S. tenuicaudatum;* these bodies contain 20–50 granules surrounding the end of the vesicle.

In the second group would fall such bodies as those of *S. arevaloi, S. glandulosum, S. virginianum, S. ciliatum, S. uronephrium, S. cryptops, S. bicaudatum,* and *S. kepneri.*

In the third group would fall such bodies as those of *S. simplex.*

According to von Graff our first type is the more prevalent type. This does not hold for the American species among which bodies of our second type are more common.

These light-refracting bodies are typically paired and vary in number from one to four pairs. The position of the bodies with reference to the cerebral ganglia is variable in different species. The most prevalent condition is one in which a single pair of such bodies is associated with the posterior lobes of the cerebral ganglia. In *S. arevaloi* there are two pairs above the anterior ganglionic lobes and one pair above the posterior ganglionic lobes. In *S. glandulosum* the four pairs of refractive bodies lie above the anterior lobes, in *S. cryptops* one pair is associated with the posterior lobes and one pair with the anterior lobes and in *S. bicaudatum* the three pairs lie above the anterior ganglionic lobes.

Another feature of considerable diagnostic value is the pharynx. The pharyngeal wall may be muscular or membranous; it may be modified so as to present two or more fairly

definite regions; the pharynx may be completely surrounded externally by unicellular glands or the glands may be localized; the radial musculature of the pharynx may be very elaborate or relatively undeveloped; the pharynx may contain special,

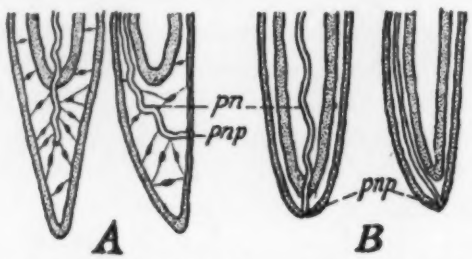

FIG. 4. Diagrammatic dorsal and lateral optical views of posterior end of *Stenostomum* showing, *A*, protonephridium opening in middle of enteron-free tail region, and *B*, terminal opening of the protonephridium in a type without appreciable enteron-free tail region. *pn*, protonephridium; *pnp*, opening of the protonephridium or protonephridiopore.

evertible tentacular structures. The oral opening may have no apparent skeletal elements or it may be equipped with a tubercle-like or rod-like skeletal element on each side.

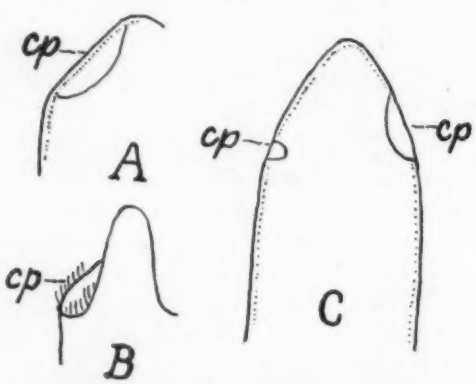

FIG. 5. Types of ciliated pits. *A* and *B* large anteriorly directed pits; *C*, small lateral pit on left and larger lateral pit on right. *cp*, ciliated pit.

The position of the posterior nephridiopore is likewise a relatively constant character. It may be located terminally or slightly subterminally (Fig. 4, *B*), or it may be placed more anteriorly between the end of the enteron and the posterior end of the body (Fig. 4, *A*).

The epidermal cells of most species contain more or less prominent rod-shaped rhabdites. In *S. arevaloi, S. ventronephrium, S. anops, S. bicaudatum* and *S. glandulosum* the epidermis contains peculiar "gland" cells and in *S. predatorium* there are large glands lying just beneath the general epidermis. These special epidermal features are sharply diagnostic.

There are, of course, other features such as the shape, size and position of the ciliated pits (Fig. 5) which are constant and which are consequently of value in specific diagnoses, but those briefly discussed above are more obvious and of more general application.

KEY TO THE AMERICAN SPECIES OF THE GENUS STENOSTOMUM

A. *Stenostomum* with light refracting bodies.
  B. Light refracting bodies, a single pair.
    C. Light refracting bodies composed of more than 10 granules or spherules.
      D. Ventral surface with longitudinal tracts free of large rhabdites ..... *grande* (p. 231)
      DD. Ventral surface without rhabdite-free tracts ..... *tenuicaudatum* (p. 235)
    CC. Light refracting bodies composed of less than 10 granules or spherules.
      D. With appreciable free caudal region.
        E. Protonephridial opening terminal or slightly subterminal ..... *uronephrium* (p. 249)
        EE. Protonephridial opening not terminal or slightly subterminal ..... *ciliatum* (p. 241)
      DD. Without appreciable free caudal region.
        E. Protonephridial opening terminal or slightly subterminal.
          F. Cilia minute, light refracting bodies without vesicles ..... *simplex* (p. 239)
          FF. General ciliary coat prominent; long cilia sparsely scattered over body; light refracting bodies with prominent vesicles ..... *virginianum* (p. 257)
        EE. Protonephridial opening not terminal or slightly subterminal; opening typically beneath enteron ..... *kepneri* (p. 264)
  BB. Light refracting bodies, more than a single pair.
    C. With a single, dorsally directed, caudal outgrowth ..... *arevaloi* (p. 251)
    CC. With ventral and dorsal caudal outgrowths and with six evertible, tentacular fingers in the pharynx ..... *bicaudatum* (p. 228)
    CCC. Without special caudal outgrowth.
      D. Four pairs of light refracting bodies lying above anterior ganglionic lobes ..... *glandulosum* (p. 243)
      DD. Two pairs of light refracting bodies ..... *cryptops* (p. 262)

AA. *Stenostomum* without light refracting bodies.
B. Without gizzard-like unit in enteron.
C. Protonephridial tube dorsal to enteron.
D. Without dorsally directed, caudal outgrowth.
E. Without large glandular bodies beneath epidermis.
F. Epidermis without very long, prominent, stiff cilia.
G. Ciliated pits small, lateral, hemispherical ..... *brevipharyngium* (p. 238)
GG. Ciliated pits large, anteriorly directed.
H. Pharynx parchment-like or membranous ..... *membranosum* (p. 240)
HH. Pharynx not parchment-like or membranous; with tubercle-like body on each side of mouth ..... *mandibulatum* (p. 242)
GGG. Ciliated pits medium or large; not anteriorly directed.
H. With epidermal "rosette" bodies ..... *anops* (p. 266)
HH. Without epidermal "rosette" bodies ..... *pegephilum* (p. 269)
FF. Epidermis bearing very long, prominent, stiff cilia.
G. With short, distinct tubercle from ventral margin of anterior tip ..... *tuberculosum* (p. 267)
GG. Without tubercle associated with anterior tip ..... *saliens* (p. 245)
EE. With large, glandular bodies beneath epidermis ..... *predatorium* (p. 246)
DD. With dorsally directed caudal outgrowth ..... *psuedoacetabulum* (p. 260)
CC. Protonephridial tube posterior to pharynx ventral to enteron ..... *ventronephrium* (p. 254)
BB. With gizzard-like unit in enteron ..... *tauricum* (p. 237)

Descriptions of Species [1]

Species of Unquestionable Validity

S. bicaudatum Kennel (Pl. VII, Fig. 2)

Kennel, 1889; Martin, 1908.

This species was described by Kennel (1889) from Trinidad. Martin (1908) described it under the name *Weldonia*

[1] Type specimens are located in the collections at the University of Virginia and at the University of Georgia. At the University of Virginia: *S. ciliatum*, *S. membranosum*, *S. simplex*, *S. mandibulatum*, *S. brevipharyngium*, *S. saliens*,

paraguayensis from South America but Luther (1908) shortly pointed out that the latter name should be relegated to synonymy.

We collected this species in large numbers about ten miles south of Valdosta, Georgia in roadside ditches. It has not been described before from North America.

Kennel's description is none too accurate but leaves no doubt as to the species with which he was dealing. We are completely redescribing the species without reference to the many corrections and additions we have made.

The maximum observed length for a specimen of 9 zooids was 7 mm. The body is heavy and thick but tapers sharply near the posterior end. Single zooids when contracted are almost spherical. There is a slight dorsal flattening. The posterior end bears two caudal outgrowths—the dorsal outgrowth is thicker and shorter; the ventral outgrowth delicate and long and is dragged after the animal. The ventral outgrowth is well-muscled and may be contracted considerably.

The epidermis, like the rest of the body, is faintly brownish. The enteron is dark and stands out prominently.

The general ciliary coat, composed of cilia of a length slightly greater than the epidermal thickness, is uniform except over the anterior end where it seems shorter and in the two narrow rhabdite-free tracts where the cilia are very fine and very active.

The short, rod-shaped rhabdites are confined to the distal ends of the epidermal cells. Rhabdites are uniformly distributed over the surface of the body except for a narrow rhabdite-free tract on each side of the mid-dorsal line. These tracts extend from the anterior end and converge at the tip of the dorsal outgrowth. Distributed over the body in the epidermis are special epidermal bodies like those of *S. arevaloi*. These bodies are not found in the mid-dorsal band bordered by the outer margins of the rhabdite-free tracts.

S. tauricum (as *S. gigerium*), *S. grande* (as *S. oesophagium*), *S. glandulosum*, *S. predatorium*. At the University of Georgia: *S. bicaudatum*, *S. tenuicaudatum*, *S. virginianum*, *S. ventronephrium*, *S. uronephrium*, *S. arevaloi*, *S. pseudoacetabulum*, *S. cryptops*, *S. kepneri*, *S. pegephilum*, *S. tuberculosum*, *S. anops*.

The cephalic ganglia conform to the generic pattern.

The light refracting bodies are 3 pairs and in general are like those of *S. arevaloi*. The anterior pair is generally composed each of a single spherule—the second pair close behind these, is also each composed of a single spherule. The posterior pair (each of 2 or 3, rarely 4 spherules) is considerably separated from the two anterior pairs and is more lateral in position. Each of the 3 pairs of refractive bodies has associated with it a clear vesicle. They all lie above the anterior ganglionic lobes.

The ciliated pits are large and deep and lateral in position.

The mouth is medioventral in position and is a longitudinal slit at the level of the posterior margins of the ciliated pits.

The pharynx is an extremely long tube when the animal is extended. Its intrinsic musculature is well developed. We have seen no cilia in its lumen. The radial musculature of the pharynx is extremely elaborate especially on the dorsal side. The distal ends of the fibers are inserted well anteriorly (especially those of the antero-dorsal end of the pharynx). A peculiar feature of the pharynx is an organ (called by Kennel a "grasping organ") bearing six finger-like, or tentacle-like, structures on its anterior face. The posterior end of the organ appears to be in the shape of two half-spheres (or a sphere with a groove) each of which bears on its anterior face three tentacular structures. The rounded base of this organ seems to be attached to the dorsal wall of the pharynx and when the pharynx is contracted the tentacles are spread as they are pushed from the mouth. The spherical posterior face of this organ is covered by long-necked, unicellular glands. Internally each tentacle appears to be composed of a series of spherical cells. Kennel (1889) suggested that these tentacles serve in the capture of food but we have not observed their use for this purpose, though it seems Kennel's conclusion is probably justified. The wall of the pharynx is apparently without glands.

The enteron extends posteriorly to the base of the ventral outgrowth. There are few gland cells in the enteric wall and these are not at all prominent.

The cells of the pseudocoel are of various sizes and for the most part are highly refractive.

The prominent protonephridium follows the usual path and opens dorsally at the base of or near the middle of the dorsal outgrowth. In most specimens granular secretions may be seen within the lumen of the tube level with the pharynx.

We have been unable to culture this species satisfactorily.

Specific diagnosis: 3 pairs of light refracting bodies; 2 caudal outgrowths; an evertible structure bearing 6 tentacles associated with the pharynx; 2 rhabdite-free tracts on the dorsal surface.

S. grande Child (Pl. V, Fig. 1)

Child, 1902*a*, 1903; Graff, 1911, 1913; Van Cleave, 1929; Kepner and Carter, 1931; Carter, 1933; Kepner, Carter and Hess, 1933; Kepner and Yoe, 1933; Nuttycombe and Waters, 1935*c*.

This species appears to be rather widely distributed. According to the literature it has been collected in New York, Massachusetts, Illinois and Pennsylvania. We have collected it in Virginia but have not been able to find it in North Carolina or Georgia. Nasanov (1925) and Steinboch (1932) have recorded this species from Russia.

Stenostomum grande was first described by Child (1902*a*). It has since been described by Kepner and Carter (1931) as *S. oesophagium*. Kepner and Carter (1931) state that the "oesophageal region" of the pharynx is constant in *S. oesophagium*, whereas, it is a variable region in *S. grande;* further *S. oesophagium* is always sexually mature (testes always present), whereas, *S. grande* becomes sexually mature during the autumn. Kepner, Carter and Hess (1933) later found sexuality did not occur in nature in *S. oesophagium* in the early autumn.

Our reasons for rejecting *S. oesophagium* as a valid species are as follows: We have maintained cultures of *S. oesophagium*, always sexually mature as males, for 4 years and have examined them repeatedly to determine whether or not the

"oesophageal region" is a constant definite region. In old specimens we have frequently found this region thrown into folds such as Child described for this part of the pharynx of his *S. grande*. We have also studied carefully a species collected at Mountain Lake, Virginia, identified as *S. grande*, which did not show gonads. We perceive no constant appreciable differences between this *S. grande* and *S. oesophagium* with reference to the "oesophageal region" or to any other feature. Further, we believe as has already been pointed out by Sekera (1903*b*), Nuttycombe (1931) and Kepner, Carter and Hess (1933), environmental conditions to be a major factor in sexuality in *Stenostomum* and we see no reason for assigning specific differences to the presence or absence of gonads at any particular time if these gonads do not differ structurally when they do appear. As a point of interest, it may be noted that the *S. oesophagium* cultured for several years by Kepner, Carter and Hess, periodically became sexually mature as females and ours, sub-cultured from their clone, under another set of culture conditions have never become sexually mature as females. All factors considered, we believe that the difference between *S. grande* and *S. oesophagium* are not sufficient to be of specific caliber. We are therefore rejecting *S. oesophagium* as a valid species. A description of *S. grande* follows:

Healthy individuals of one zooid measure about 1.9 mm. The length of single-zooid specimens is highly variable because of the fact that a newly formed individual from an anterior zooid may have a very short, free tail region, whereas, such an animal formed from a posterior zooid will have a long, free tail region. Single zooid individuals are rarely encountered in healthy cultures. Our longest zooid-chains measure as much as 6.0 mm. in length.

In reflected light the animal is white except for the enteric region which in well-fed specimens may be orange. In transmitted light the epidermis is faintly yellowish or brownish, the pseudocoel translucent, the pharynx brownish and the enteron, dependent upon its food content, orange to brown with a generally granular appearance.

From above the body has the appearance of a cylinder tapered at both ends. In front of the mouth the body tapers rather abruptly. Posteriorly tapering begins somewhat anterior to the caudal end of the enteron and proceeds posteriorly to the narrow, free tail region. This free tail region is sometimes equal to 1/4 the body length. The posterior half of the ventral body wall is flattened or even concave. This part of the ventral surface is highly adhesive.

The general ciliary coat is uniform—the length of these cilia is about the same as the epidermal thickness. Long, semi-rigid cilia, 2 or 3 times as long as those of the general ciliary coat, are sparsely distributed over the body.

The numerous, large, rod-shaped, epidermal rhabdites are disposed perpendicularly or at a slight angle. They may occur singly or in groups. In the slightly contracted state of the epidermis the rhabdite groups become cone-shaped with the apex of the mass inward. In the dorsal and lateral regions the distribution of the rhabdites is fairly uniform but on the ventral surface there are three longitudinal tracts free of large rhabdites—a wide median tract, extending from the mouth posteriorly and a narrow tract on each side of this wide median tract.

The ciliated pits are lateral, extremely large and elastic and somewhat wider at their posterior margins.

The nervous system conforms well to the pattern for the genus.

There is a single pair of light-refracting bodies. Each body is dish-shaped, composed of many refractive granules or spherules borne on the posterior face of a vesicle closely associated with the posterior ganglionic mass on its respective side. The nervous system and refractive bodies are very similar to those of *S. tenuicaudatum*.

The mouth is extremely large and its most characteristic shape, at rest, is roughly rectangular. The "lips" are mobile and are capable of such enormous stretching that the oral opening may be extended until it considerably exceeds the usual diameter of the body.

The muscular pharynx is long and the posterior portion shows various degrees of folding. This is the oesophageal region and there is generally an appreciable constriction between it and the anterior pharyngeal region. The anterior portion is covered externally by unicellular glands—the posterior region is free of glands. Histologically the posterior region shows a much thinner wall and its epithelium has a much lighter ciliary coat than that of the anterior region (Nuttycombe and Waters 1935*c*).

Most of the cells of the enteric epithelium are finely granular and show refractive droplets of various sizes. Clear vacuoles are very prominent in starved specimens. In addition to these enteric cells are found fairly numerous cells ("gland" cells) which show masses of larger refractive granules. The internal margins of the enteric cells are finely ciliated.

The pseudocoel contains a variety of cells as to size, shape and color. The cells of the pseudocoel in the anterior and posterior regions of the body stand out sharply.

The radial musculature of the pharyngeal region is fairly well-developed but does not approach the strength of the corresponding musculature in *S. virginianum*, *S. predatorium* or *S. bicaudatum*. The radial musculature in other parts of the animal is relatively well-developed.

The prominent protonephridial tube passes from beneath the transverse commissure posteriorly over the pharynx and enteron by an undulatory path. The ventral nephridiopore varies in its location from about half-way between the end of the enteron and the end of the pseudocoel to almost the posterior tip of the pseudocoel. Its usual point of exit is near the middle of the free caudal region.

When the male gonad is present it appears as a rather large, lobulated structure located above the anterior pharyngeal region.

This species feeds primarily upon large masses of material such as bacterial glea or sediment containing small plants (algae, etc.) and small animals (protozoa, rotifers, etc.). It

cultures easily in wheat-grain infusion but its reproduction is rather slow.

Specific diagnosis: 1 pair dish-shaped, light-refracting bodies each composed of many (more than 20) spherules; protonephridiopore near middle of caudal triangle; pharyngeal glands restricted to anterior part of pharynx; 3 tracts on the ventral surface posterior to the mouth free of large rhabdites.

S. tenuicaudatum v. Graff (Pl. IV, Fig. 1)

Graff, 1911, 1913; Kepner and Cash, 1915; Van Cleave, 1929; Nuttycombe 1932*a*; Van Cleave, 1929, 1934.

This species is probably the most widely distributed species of the genus in the United States. It has been reported from the whole Atlantic seaboard and the Midwest. We have found it very prevalent in the 5 Southern States (Virginia, North Carolina, South Carolina, Tennessee, and Georgia) in which we have collected. Nasonov (1926) and Beklemischev (1917) reported it from Russia. It is also probably the easiest of all species to culture in wheat grain infusion.

Our type variety of this species has a maximum observed length of 5.0 mm. for a chain of 8 zooids. The usual number of zooids is 3 to 5 with a length of 1.5 to 2.5 mm. Single underfed zooids may measure as little as 0.3 mm.

The body is roughly cylindrical but is somewhat flattened ventrally. The anterior end is bluntly rounded; the posterior end tapers gradually from near the posterior end of the enteron to the somewhat pointed caudal tip. The enteron-free caudal region varies in length but is always considerable if the specimen is not newly formed from an anterior zooid. This caudal region is highly adhesive on the ventral side.

In reflected light the color is milky white except for the enteron which may appear somewhat dark. In transmitted light the enteric region is yellowish to dark brown and the rest of the body is translucent to brownish.

The general epidermal ciliary coat is fine and heavy—the cilia are approximately as long as the epidermal thickness. Occasional long, fine, semi-rigid cilia are scattered over the body.

The rhabdites of the epidermal cells are rod-shaped, disposed perpendicularly to the epidermis and may occur singly or in packets.

The ciliated pits are large, dorso-lateral in position and wider at their posterior margins.

The nervous system conforms to the pattern of the genus.

There is a single pair of dish-shaped light-refracting bodies, each of which is composed of 20–50 granules or spherules covering the posterior face of a vesicle closely associated with an outgrowth from the posterior ganglionic lobe on its respective side.

The mouth at rest is crescent-shaped. Its "lips" are fairly mobile.

The pharynx is rather short and broad. The pharyngeal wall contains large bands of longitudinal fibers which are very prominent at the points of convergence around the mouth. Circular fibers are not so prominent. The outer pharyngeal wall is covered almost to its posterior end by large unicellular glands. The pharyngeal lining is ciliated. The pharyngo-enteric sphincter is located somewhat below the center of the anterior enteric wall.

The enteric tube conforms to the body shape in the region in which it lies. It ends rather bluntly rounded, somewhat anterior to the posterior part of the pseudocoel leaving an enteron-free caudal region. This caudal region in one variety of the species is seldom more than 1/8 the total body length, whereas, in the others this region is characteristically about 1/3 the entire body length. The enteric lining is ciliated. The cells of the enteric epithelium contain many small granules and vacuoles. Occasional cells which contain masses of transparent refractive granules (usually much larger than the scattered granules of the other enteric cells) stand out very prominently except when the enteric contents are dark. The cells containing these granular masses are called, in most descriptions, "gland cells" but we have not been able to find any demonstration of their function. The number of granules in such a mass is variable.

The general parenchyma contains cells of various sizes and shapes. Many of the larger cells are finely granular.

The radial musculature is quite evident in the pharyngeal and caudal regions but is less prominent in the enteric region.

The protonephridial tube passes posteriorly from beneath the transverse brain commissure dorsally over the pharynx and enteron. Posterior to the end of the enteron it is generally thrown into several loose coils and finally opens ventrally about (or a little anterior) half-way between the posterior enteric tip and the end of the pseudocoel.

Reproduction is asexual as a rule. In nature, sexually mature females have been collected several times in the fall (October and November). We have a clone which has been cultured continuously for eleven years but no sexually mature individuals have ever appeared in it.

Specific diagnosis: 1 pair dish-shaped, light-refracting bodies each composed of many (more than 20) spherules; protonephridiopore near the middle of a definite enteron-free caudal region; pharynx almost covered by large unicellular glands; rhabdites uniformly distributed on the ventral surface.

S. tauricum Nasonov (Pl. II, Fig. 1)

Nasonov, 1924; Kepner and Carter, 1931.

Nasonov (1924) described this species from Crimea; Kepner and Carter (1931) described it from the vicinity of the University of Virginia under the name *S. gigerium* and we have collected it in large numbers near Valdosta and Athens, Georgia.

The description which follows is that of Kepner and Carter. "Specimens usually show well advanced fission planes. Such specimens measure about 1 mm. in length. The slender body is spindle-shaped, its greatest width being at the level of the posterior end of its anterior third. The body bears a dense uniform coat of small cilia. The oval ciliated pits are dorso-laterally placed. There are no light-refracting bodies. The anterior lobes of the cephalic ganglia are relatively long. The commissure of the cephalic ganglia

lies dorsal to the anterior margin of the mouth. The relatively large posterior lobes of the cephalic ganglia extend posteriorly by the sides of the pharynx. The mouth is short and somewhat fan-shaped. The short pharynx is conical. The enteron extends back almost to the posterior end of the body. The anterior region of the enteron is interrupted by the development of a gizzard. It is the presence of this organ that has suggested the name for the species. The slender protonephridium opens ventrally somewhat nearer the posterior end of the enteron than the tip of the caudal region. Many of these animals have been maintained for over a week in a wheat infusion that supported many protozoa. Specimens for in toto mounts have been well fixed in Zenker's fluid."

This species cultures readily in wheat grain infusions. Most of the specimens which we collected showed euglenoid organisms in the pseudocoel.

Specific diagnosis: Light-refracting bodies absent; protonephridiopore not terminal or subterminal; pharynx short and conical; anterior end of enteron equipped with a gizzard.

S. brevipharyngium Kepner and Carter (Pl. II, Fig. 2)

Kepner and Carter, 1931, Tseng-Jui Tu, 1934.

The description which follows is that of Kepner and Carter (1931). We have not encountered this species in our collecting.

"Specimens studied were collected from the mud in the overflow from Edgemont Dairy water-basin on the south side of the Observatory Mountain, July 10, 1929." The Observatory Mountain referred to is about two miles southwest of the University of Virginia. Tseng-Jui Tu (1934) records this species from China.

"The many specimens that appeared in our aquarium after the latter had stood in laboratory for five days, all showed at least one fission plane. A specimen, with a single obvious fission plane, measures about 0.6 mm. The body is spindle-shaped tapering less abruptly posteriorly than anteriorly. Epidermal cilia are short and uniform in size and

distribution. Ciliated pits are minute and hemispherical. There are no light-refracting bodies. The slender, relatively long mouth is longitudinally disposed. This mouth is guarded by lateral lips that present, in the living specimen, definite contours. Within each lip there lies a skeletal-like rod which has a texture similar to that of chitin. Chitinous material, however, could not be demonstrated. The pharynx extends but a little distance beyond the posterior margin of the mouth. The enteron is continuous posteriorly almost to the end of the body. The contents of the enteron are finely granular, suggesting that this species feeds upon bacteria and naked protozoa. The mesenchyme appears refractive by transmitted light. Little can be made out of the details of the living mesenchyme."

Specific diagnosis: Light-refracting bodies are absent; protonephridiopore ventral and subterminal; pharynx extremely short; each lateral lip of mouth with a skeletal rod; ciliated pits minute and hemispherical.

S. simplex Kepner and Carter (Pl. II, Fig. 3)

Kepner and Carter, 1931.

The description which follows is that of Kepner and Carter (1931). They collected this species in the vicinity of the University of Virginia. We have not encountered this species in our collecting.

"Specimens have not been found that did not possess two or more advanced fission planes. One with three fairly conspicuous fission planes measures about 1 mm. in length. The cylindrical body tapers gradually posteriorly and rather abruptly at the anterior end so as to be somewhat cigar-shaped. Epidermal cilia minute. Ciliated pits very small. Associated with each posterior lobe of the cephalic ganglia is a simple granule or light refracting body. This body measures about 2 μ in diameter. It does not have an attending vesicle. These simple refractive elements lie opposite the anterior margin of the mouth. These simple bodies have suggested the specific name. In total mounts, the transverse com-

missure of the cephalic ganglia appears almost spherical, the anterior face being somewhat more convex than the posterior one. The short, but elongated mouth is disposed parallel to the axis of the body. The pharynx is not conspicuously glandular. The enteron extends to near the posterior limit of the body. The protonephridium is slender and opens at the posterior end. This species frequently appears in dense aquarium infusions some days after the material has been collected. Specimens were well fixed, for in toto mounts, in Zenker's fluid."

Specific diagnosis: 1 pair of light-refracting bodies each composed of a simple granule; protonephridiopore terminal; pharynx not conspicuously glandular; epidermal cilia minute; ciliated pits very small; no appreciable enteron-free caudal region.

S. membranosum Kepner and Carter (Pl. I, Fig. 2)

Kepner and Carter, 1931.

The description which follows is that of Kepner and Carter (1931). We have not encountered this species in our collecting.

"On October 16, 1929, material from the swampy soil below the spring near top of Mountain Elliot, Augusta County, Va., was placed in an aquarium. The next day many specimens of this species appeared beneath the surface of this aquarium. Specimens were very well fixed, for in toto mounts, in Zenker's fluid.

"Specimens, lacking obvious fission planes, measure about 0.5 mm. in length. Body spindle-shaped, colorless. Rhabdites absent. Epidermal cilia minute. Cilia of ciliated pits conspicuously long. No light refracting bodies. Anterior lobes of cephalic ganglia much larger than the posterior ones as shown by permanently mounted specimens. Mouth, in free swimming specimen, elongated. Pharynx parchment-like or membranous in appearance. This feature has suggested the specific name. A conspicuous muscular labial sphincter appears in our permanent in toto mounts. Enteron extends

almost to posterior end of body. Protonephridium is very slender, opening at posterior end of body. Only chains of two zooids have been observed."

Specific diagnosis: Light-refracting bodies absent; protonephridiopore terminal; pharynx parchment-like or membranous in appearance; rhabdites apparently absent; prominent cilia associated with ciliated pits.

S. ciliatum Kepner and Carter (Pl. VIII, Fig. 1)

Kepner and Carter, 1931.

The description which follows is that of Kepner and Carter (1931). They collected this species in the vicinity of the University of Virginia. We have not encountered this species in our collecting.

"Specimens, without obvious fission planes, measure 1 mm. or less. The body is stoutly spindle-shaped tapering posteriorly into a rather slender caudal region. Epidermal cilia small and uniformly distributed. Ciliated pits relatively small. Associated with each posterior lobe of the cephalic ganglia is a colorless vesicle that contains on its posterior floor one, two or three more or less rod-shaped light refracting bodies. The narrow mouth is transversely disposed. The pharynx is wide; has a thin wall and a relatively large lumen. Within this lumen numerous long cilia undulate. These large pharyngeal cilia have suggested the specific name. The enteron extends posteriorly to the base of the tapering caudal region of the body. This form is much given to ingesting muck. The slender protonephridium opens externally on the ventral surface a short distance posterior to the fundus of the enteron. A culture was established with four wild specimens on March 16, 1929. On May 1, 1929, all of the many stenostoma, that were examined from this culture, resembled the four original ones. Specimens were well fixed, for in toto mounts, in Zenker's fluid. Specimens thus fixed laid down about themselves a mucus-like deposit such as *S. tenuicauda* deposits about itself under adverse conditions."

Specific diagnosis: 1 pair light-refracting bodies each composed of 1, 2, or 3 flattened granules; protonephridiopore not terminal; wide, thin-walled pharynx with heavily ciliated inner epithelium; fairly extensive enteron-free caudal region.

S. mandibulatum Kepner and Carter (Pl. VIII, Fig. 2)

Kepner and Carter, 1931.

The description which follows is that of Kepner and Carter (1931). They collected the species in the vicinity of the University of Virginia. We have not encountered this species in our collecting.

"Individuals without obvious fission planes measure about 0.33 mm. in length. The width of the body is relatively somewhat greater than that of other species. The epidermal cilia are relatively long and are uniformly distributed. The anterior end of the body is abruptly tapered. Upon the oblique anterior margins of the body lie the ciliated pits. The ciliated pits in this form are more anteriorly directed than in other forms. There are no light refracting bodies. The mouth is V-shaped. Each arm of the V-shaped lip has a refractive tubercle-like body near its anterior end and the diverging lips opened and closed in a manner that suggested that the tubercle-like bodies might have been chitinous mandibles. Chitin tests, however, showed no such material present. The mandible-like motion of the lips suggested the species name. The enteron extends posteriorly to near the end of the body. Only chains of two zooids have been observed. This species made its appearance in great numbers in an old, but not foul, infusion, September 9, 1929. Great numbers of them, sexually mature as females, continued to live in this aquarium throughout September. The ovary in this species is relatively large. Specimens were fixed well, for in toto mounts, in Zenker's fluid."

Specific diagnosis: Light-refracting bodies absent; each side of V-shaped mouth with refractive tubercle-like body near its anterior end; ciliated pits large and anteriorly directed.

S. glandulosum Kepner and Carter (Pl. III, Fig. 1)

Kepner and Carter, 1931.

This species is apparently rather widely distributed. We have collected it from Virginia, North Carolina and Georgia.

The body in specimens of several zooids appears thread-like so great is the length in comparison with the diameter. Even in an individual showing one or two fission planes the ratio of length to diameter is about 1 to 20. According to Kepner and Carter this species reaches a maximum length of 12 mm. and may show as many as 16 zooids.

The body is roughly cylindrical. The anterior end, in front of the mouth, is highly mobile and is continually changing shape, extending and retracting. At rest the anterior end is bluntly rounded. The caudal region tapers gradually and ends bluntly rounded or truncated.

The color in reflected light is milky white. In transmitted light the epidermis is translucent and shows many, nearly colorless, refractive masses ("rosette cells"); the enteric region is translucent or grayish and finely granular. The enteric wall may show occasional cells bearing masses of refractive granules and occasional clear vacuoles. The region anterior to the enteron is faintly brownish.

The general ciliary coat is uniform; the cilia are approximately as long as the epidermal thickness.

Kepner and Carter (1931) state that this species lacks rod-shaped rhabdites in the epidermis. We have observed fairly numerous, small rhabdites in the head region and have found extremely tiny rhabdites scattered over the rest of the body. As observed by Kepner and Carter the epidermis contains "rosette-shaped gland cells." Each rosette is composed of 1, 2, 3, or 4 large, clear, refractive vesicles. Kepner and Carter found that products from the rosettes had a lethal effect upon the animal's prey. These rosettes are relatively scarce in the region anterior to the level of the transverse cerebral commissure.

Kepner and Carter described paired ventral, lateral and dorsal ciliated pits. The ventral and lateral pits are large and

prominent but we have not identified the dorsal pits in living specimens unless they be two ciliated grooves on each side of the mid-dorsal line.

The nervous system conforms to the general pattern for the genus. The anterior lobes of the brain are unusually long.

There are no light-refracting bodies associated with the posterior, cerebral lobes but there are, dorsal to the anterior lobes, refractive granules which are generally so arranged that the units above the left, anterior cerebral lobe are paired with corresponding units on the right side. Kepner and Carter reported 4 such pairs of granules. Whereas, the spacing is usually regular between the bodies, we have found considerable irregularity in the number and exact position of these refractive bodies and we have further found that the individual "bodies" may be composed of 2 granules or spherules.

The mesio-ventrally placed mouth is, when at rest, roughly crescent-shaped—the powerfully-muscled oral margin is highly mobile and various shapes occur.

The pharyngeal wall is highly muscular. The posterior, outer surface of the pharynx is covered by long-necked gland cells the stems of which apparently open into the anterior part of the pharynx. The pharyngeal epithelium is heavily ciliated.

The outer margin of the enteric tube is, in optical section, somewhat dentate. The enteric cells are usually heavily laden with diffusely scattered granules and vacuoles of various sizes. Occasional "gland cells" containing masses of granules occur. The enteric lining is finely ciliated. The enteron extends posteriorly almost to the end of the pseudocoel.

The body wall is unusually well-muscled and is subject to frequent waves of contraction.

There are very few free-cells in the pseudocoel in the enteric region but a few very large free-cells may generally be seen in the anterior portion of the pseudocoel. The radial musculature stretching from the body wall to various internal parts is not very well developed.

The prominent protonephridial tube passes from beneath the transverse commissure posteriorly by a tortuous path above the pharynx and enteron. Its pore is terminal or subterminal.

Kepner and Carter state that members of this species are scavengers, are sometimes predatory and will not live in the presence of an abundance of algae. We have cultured this species with very considerable difficulty in wheat-grain infusions of protozoa but have reared it with great ease when fed on the small variety of *S. virginianum*.

Specific diagnosis: 4 pairs (typically) of light-refracting bodies associated with the anterior lobes of the cerebral ganglia; protonephridiopore terminal; more than 1 pair ciliated pits; "rosette" bodies in epidermis.

S. saliens Kepner and Carter (Pl. V, Fig. 2)

Kepner and Carter, 1931.

The following description of *S. saliens* is drawn largely from that of Kepner and Carter (1931). We have added a few descriptive notes from our studies of several specimens of this species collected at Athens, Georgia and Mt. Lake, Virginia. Kepner and Carter collected this species at the University of Virginia.

Specimens without obvious fission planes measure about 0.62 mm. in length. The body is spindle-shaped; its greatest width is in the vicinity of the pharynx and mouth. The ventral surface is considerably flattened.

The general ciliary coat is uniform and dense. The length of these cilia is somewhat greater than the epidermal thickness. Sparsely distributed over the body are prominent, slender, stiff, very long cilia. These apparently enable the animal to "leap" suddenly. Kepner and Carter (1931) observed *Euplotes* collide with the anterior end of *S. saliens* following which the latter would leap after and catch the hypotrich. "It is its ability to leap that has suggested the specific name for this animal."

There are no epidermal rhabdites of the conventional rod-shaped type but the cells contain isolated granules or globules of considerable size. These do not appear to be rhabditic in character.

The "brain" is much like that of *S. cryptops.*

The ciliated pits are small and hemispherical.

There are no light-refracting bodies.

The mouth is elongated and has definitely delimited anterior, posterior and lateral "lips." The anterior "lip" is less muscular and less mobile. Heavy muscle bands are very evident in the lateral and posterior "lips." The lateral "lips" are incessantly opening laterally and closing, much as might the mandibles of an arthropod. Only a very small portion of the pharynx lies posterior to the posterior "lip." There are no obvious pharyngeal glands.

The enteron extends to near the posterior end of the body. The general enteric epithelium is coarsely granular. There may also be seen in this epithelium a few "gland cells" containing compact masses of larger granules. The free ends of the enteric cells are ciliated.

The protonephridiopore is located ventrally and subterminally.

This species cultures easily in wheat-grain infusions of small protozoa.

Specific diagnosis: Light-refracting bodies absent; subterminal protonephridiopore; very little pharyngeal region posterior to caudal oral margin; powerfully muscled lateral and posterior lips; ciliated pits small, hemispherical; sparse coat of prominent, long, semi-rigid cilia.

S. predatorium Kepner and Carter (Pl. IV, Fig. 3)

Kepner and Carter, 1931; Nuttycombe and Waters, 1935*c*.

This species has been collected in Virginia, North Carolina and Georgia. Plotnikow (1905*b*) describes a *Stenostomum* species from Russia which we believe is *S. predatorium.*

Well-fed, single-zooid specimens of *S. predatorium* measure about 1 mm. in length. The maximum observed length was

4.5 mm. for a chain of 9 zooids. We have found an extremely small variety of this species at Mt. Lake, Virginia.

The body is roughly a cylinder tapered at both ends. The anterior end tapers less gradually. There are slight constrictions just anterior and posterior to the pharyngeal region.

In reflected light the color is milky white with a slightly darkened enteric region. In transmitted light the animal is translucent except for the enteron which may be almost black in well-fed specimens. The epidermis and region anterior to the enteron have a slightly brownish tinge.

The general ciliary coat is dense and uniform. The length of the cilia is about the same as the epidermal thickness. Semi-rigid cilia about 2.5 times as long as those of the general ciliary coat are rather freely distributed over the body.

No rhabdites have been identified. The epidermal cells in the region anterior to the pharynx contain tiny granules just beneath their surfaces but these have not been definitely identified as rhabdites.

Lying sunken beneath the epidermis are large glands which, according to Kepner and Carter (1931), "discharge a colloidal material that has the power to paralyze other stenostoma." These large depressed glands occur in no other described American species.

The two ciliated pits are of medium size.

The nervous system conforms to the generic pattern.

There are no light-refracting bodies associated with the cerebral ganglia.

The mouth, at rest, appears as a longitudinal slit somewhat wider at its posterior end. The anterior and posterior "lip" regions remain at the level of the body surface, whereas, the lateral "lip" margins are depressed below the body surface. When the mouth is widely opened the lateral regions are elevated and become continuous with the anterior and posterior regions at the level of the body surface. When the "lips" are thus extended the mouth opening is approximately circular. The oral sphincter associated with the mouth is extremely powerful.

The extremely long pharynx of *S. predatorium* is the most muscular to be found in the American species of the genus. The general intrinsic pharyngeal musculature is well-developed but it is the radial musculature of the pharyngeal region which is most elaborate. Nuttycombe and Waters (1935*c*) have studied this remarkably muscled pharynx in connection with the feeding habits of this and other species. Prominent, unicellular glands are located at the anterior and posterior ends of the pharynx but the mid-pharyngeal region is devoid of gland cells. We have been unable to find cilia on the pharyngeal epithelium. At rest the anterior mouth margin is just posterior to the extreme anterior end of the pharynx so there is a slight preoral pharyngeal region.

The finely ciliated enteric epithelium varies in thickness but is usually somewhat thicker than that of the epidermis. The enteric cells, when laden with food, are almost black but in starved specimens this gives place to tan or gray. The enteric epithelium contains numerous, large, highly refractive vacuoles. In general the contents of the cells are highly granular. The granular inclusions are usually diffusely scattered but occasional granular masses occur. Presumably these correspond to such masses in the "gland cells" of the other species even though the granules in *S. predatorium* are much smaller than is usual. The enteron extends almost to the posterior end of the pseudocoel leaving little free tail.

The free cells of the pseudocoel are not numerous. The radial musculature, other than that of the pharyngeal region, is not prominent.

The protonephridial tube passes posteriorly from beneath the transverse commissure of the brain dorsal to the pharynx and enteron. It opens at the posterior end of the body.

The food of this species consists almost entirely of other species of *Stenostomum*. Its feeding habits are unique. *S. predatorium*, by virtue of its powerful oral sphincter and elaborate radial, pharyngeal musculature, is capable of applying its mouth to the surface of the body of an individual of another species, of dilating its pharynx so as to suck in the

part of the body to which the mouth is applied and then, by the contraction of the oral sphincter, of pinching off the part sucked into the oral cavity. The piece is then passed through the pharynx into the enteron.

We have maintained cultures of *S. predatorium* for 3 years by feeding them on *S. tenuicaudatum*. With ample food this species cultures very readily.

Specific diagnosis: Light-refracting bodies absent; protonephridiopore terminal; extremely long pharynx with very elaborate radial musculature; glands limited to anterior and posterior ends of pharynx; large epidermal glands which extend inward into the pseudocoel.

S. uronephrium Nuttycombe (Pl. IV, Fig. 2)

Nuttycombe, 1931.

This species was collected from ponds at several widely separated points in Georgia. It has never been collected elsewhere.

Specimens without well-defined fission planes average 0.74 mm. in length; individuals of maximum length average 1.35 mm. The observed range was 0.64 to 1.5 mm. in length. The body is roughly cylindrical in shape but faintly flattened ventrally. Posteriorly there is a gradual but marked tapering and the slender caudal region ends somewhat bluntly. The extremely long tail, frequently one third and occasionally one half the total length of the body, is characteristic of fully developed specimens. Anterior to the mouth the body tapers sharply and ends bluntly rounded. This anterior region is very mobile and is constantly changing shape.

In reflected light the animal appears milky white in color. In transmitted light the body is generally translucent except for the enteron which varies from faintly yellowish to light brown dependent upon the food content. The granular "gland cells" of the enteric epithelium stand out prominently.

The general ciliary coat of the epidermal epithelium is uniform. The length of these cilia slightly exceeds the thickness of the epidermis and frequently is almost twice that of the

epidermal thickness. Very long, extremely fine cilia are sparsely distributed over the body but are fairly numerous on the anterior and posterior ends. The small, rod-shaped rhabdites, which are largely confined to the distal ends of the cells, are particularly evident in the epidermis of the caudal region.

The cephalic ganglia conform to the general pattern for the genus except for the compactness of the anterior lobes and the unusually deep lateral indentations of the posterior lobes.

The lateral ciliated pits are small and shallow and in many positions of the head are seen with difficulty.

A pair of light-refracting bodies lies at about the level of the posterior margin of the mouth. These are composed of 2 to 4 flattened or spherical granules applied to the face of a vesicle.

The mouth is medio-ventral in position and is located near the anterior end. Its marginal contour is variable even at rest but is usually nearly circular.

The highly muscular pharynx is lined with a finely ciliated epithelium. When at rest its lumen is dilated somewhat just posterior to the mouth. Numerous muscle fibers radiate prominently from the outer pharyngeal wall to the body wall. The nuclei of these fibers stand out very sharply. Small unicellular glands are sparsely scattered over the outer pharyngeal wall.

The pharynx opens immediately into the enteron the lining of which is finely ciliated. Posteriorly the enteron tapers and ends bluntly rounded. The enteric cells are apparently not sharply defined, vary in general color depending upon the food, and show large irregularly located vacuoles. The granular "gland cells" stand out very prominently among the enteric cells.

The pseudocoel contains free cells of various sizes and shapes.

The protonephridium is rather prominent and passes from the extreme anterior end under the commissure and thence along the mid-dorsal line in a slightly irregular path to near

the posterior end of the enteron where it bends abruptly to the right and downward (this curvature has been constant in all of 50 healthy specimens examined for it) and passes beyond the posterior end of the enteron lateral to it. The tube now bends sharply to the left and extends posteriorly by way of numerous coils. Active cilia may be seen within the lumen of the tube in the region of the coils. The tail region, which is highly adhesive on its ventral surface, may be greatly elongated while being used as an anchor but it is only during extreme elongation that the coils are eliminated by stretching. The protonephridiopore is terminal. The prominence of the protonephridial tube in the caudal region suggested the specific name.

The species cultures readily in wheat-grain infusions of protozoa and small rotifers but rotifers under these conditions appear to make up the bulk of the food. Mass cultures have been maintained for 6 years. Division occurs in healthy animals about once in 18 to 24 hours. In our experience, the species becomes sexually mature as females in stagnant cultures. So far no male gonads have been observed.

We have isolated some 150 of the eggs but none of these hatched.

Specific diagnosis: 1 pair of light refracting bodies composed of not more than 4 granules each; terminal or subterminal protonephridial opening; an extremely long enteron-free caudal region in which the protonephridial tube is elaborately coiled.

S. arevaloi Gieysztor (Pl. III, Fig. 2)

Gieysztor, 1931; Nuttycombe, 1932*b*.

Gieysztor (1931) collected and described this species from Spain. We have collected it a number of times from the Piedmont and Coastal Regions of Georgia and from Virginia. Healthy specimens without apparent fission planes measure about 0.75 mm. in length by 0.13 mm. in diameter (measurement just posterior to pharynx). The minimum observed length was 0.35 mm. and the maximum 2.5 mm. This

species frequently shows four fission planes and a maximum of seven has been observed in several specimens.

The body is roughly cylindrical in shape but somewhat compressed ventrally. Anteriorly there is little change in the diameter and the extreme anterior end is bluntly rounded. Posteriorly there is a marked tapering and the caudal region is usually well pointed. A peculiar feature of the caudal region is that its posterior end is bent sharply upward and is not in contact with the substratum as is the more anterior caudal region the ventral surface of which is highly adhesive. The formation of this caudal tip is unusual in that it is an outgrowth from the postero-ventral body wall of the anterior zooid and in that it is well developed before separation occurs. After separation of the zooids the outgrowth becomes dorsally directed, apparently through a twist in the caudal region.

In reflected light the animal appears milky white. In transmitted light the body is generally translucent and colorless or very faintly brownish except for the enteron which is brownish and shows brownish and greenish granular inclusions.

The general ciliary coat is uniform. The length of the cilia is slightly greater than the thickness of the epidermis. Long, semi-rigid cilia seem to be rare although occasional cilia of this type have been seen in the caudal region.

Very tiny, short, rod-shaped bodies, located just beneath the surface of the epidermal cells, appear to be minute rhabdites.

The epidermis contains numerous clear, highly refractive bodies. Each body is typically composed of several vesicles, of various sizes, so arranged as to take a horse-shoe shape. The shape, however, is by no means constant and changes with the varying stresses placed on the epidermis. These bodies are much more numerous posterior to the head region.

The ciliated pits are rather large and somewhat anteriorly directed.

The cephalic ganglia conform to the general pattern for the genus. Both anterior and posterior lobes are well defined and the mesial portions of the posterior lobes lie over the mouth.

The light-refracting bodies are peculiar. There are three pairs of such bodies in the head region just beneath the epidermis. Each is associated with a clear vesicle. The posterior pair is typically composed of two spherules each, apparently fused together, lying above the posterior cephalic lobes at about the level of the mouth. In several instances one of these posterior units has contained three spherules. The middle pair, each member a single spherule, lies just anterior to the posterior margin of the ciliated pits above the anterior cephalic lobes. These spherules are about the same size as those involved in the posterior bodies. Slightly in front of the middle pair lies the anterior pair, each a single spherule, about half the size of the spherules of the middle and posterior units. Sometimes one of these anterior bodies is displaced so as to lie mesiad or antero-mesiad to the middle body on the same side.

The mouth is mesio-ventral in position and is located near the anterior end beneath the posterior cephalic ganglia.

The pharynx is muscular and its lining is ciliated. The latero-anterior pharyngeal region lies under the posterior cephalic lobes. The outer pharyngeal wall is covered with glands similar to those of *S. tenuicaudatum*. Posteriorly the pharynx is sharply constricted and opens through the antero-ventral enteric wall.

The enteric walls follow the outline of the body posteriorly. The enteron ends bluntly rounded some distance anterior to the end of the body. The enteric epithelium is finely ciliated. The enteron is variable in color depending upon the food-content but is usually brownish. Almost invariably the enteric region just back of the pharynx shows very dark, fine, granular inclusions which cause this portion to stand out as an extremely dark region. The enteric wall contains varying numbers of vacuoles and darker granular masses.

The general mesenchyme is represented by cells of various sizes and shapes. These are rather plentiful in the region anterior to the enteron, not plentiful in the enteric region, and again rather plentiful in the free caudal region. For the most

part these mesenchymal cells are fairly large and translucent with frequent minute granular inclusions.

Radial muscle fibers are not very prominent but may be seen running from various internal parts to the body wall.

The protonephridium is not readily seen in the region anterior to the mouth but appears to pass from beneath the ganglia posteriorly over the pharynx and enteron in a waving path. The protonephridial lumen in the pharyngeal region is highly ciliated and frequently contains elonaged masses made up of transparent granules and stringy material. At the posterior end of the enteron the tube usually goes into several coils, although it is occasionally almost straight, and passes posteriorly in the free caudal region to open ventrally not farther back than the mid-point between the posterior end of the enteron and the posterior end of the body. Most frequently the pore is just back of the enteron.

The species may be cultured with difficulty in wheat infusions of small rotifers and protozoa. It may be easily cultured on the small variety of *S. virginianum*.

Sexual reproduction has not been observed in cultures although large numbers of individuals have been under observation for ten months. Division may occur as frequently as every 18 hours.

Specific diagnosis: This species has the following distinctive combination of features: 2 pairs of light-refracting bodies dorsal to the anterior cerebral ganglionic lobes and 1 pair dorsal to the posterior ganglionic lobes; protonephridiopore not terminal or subterminal; a dorsally directed caudal outgrowth.

S. ventronephrium Nuttycombe (Pl. VI, Fig. 1)

Nuttycombe, 1932*b*.

This species has been collected in Virginia and from Bulloch County, Georgia.

Healthy specimens without apparent fission planes measure about 0.5 mm. in length by 0.07 or 0.08 mm. in diameter (measurement just posterior to pharynx). The minimum

observed length was 0.3 mm. and the maximum 2 mm. Specimens of more than 3 zooids have not been observed.

The body is cylindrical. In most specimens lateral constrictions between the posterior margins of the ciliated pits and the mouth set the head off rather sharply and give to the head region a somewhat spatulate appearance from above. The anterior end is smoothly rounded in outline and is fairly mobile but is not subject to sharp changes in shape during stretching. There is a gradual tapering posteriorly and the body ends rather bluntly. The free caudal region is rarely of an appreciable length.

In reflected light the animal is milky white in color. In transmitted light the general coloration varies considerably. The epidermis is usually translucent and colorless but is sometimes yellowish. In the epidermis appear two distinctive features. (1) In the cephalic region are found large, rhabdite-like structures, which are frequently grouped into packets, that stand out prominently and give the head a warty appearance. Rhabdites other than those of the cephalic region are not noticeable. (2) In the epidermis back of the cephalic region there are numerous, large, somewhat irregularly-shaped, granular bodies which stand out sharply. Similar bodies occur occasionally in the pseudocoel and are rarely inclusions of the enteric wall.

The pharyngeal region is usually brownish and the enteric region varies from faint (not infrequently lighter than the pharyngeal region) to dark brown depending upon the food-content.

The general ciliary coat of the epidermal epithelium is uniform. The length of the cilia is approximately that of the thickness of the epidermis. There are long, semi-rigid cilia, 2 to 3 times the length of the general cilia, sparsely scattered over the body but somewhat more plentiful in the anterior and posterior regions.

The cephalic ganglia are two masses and the transverse connective is heavy.

The ciliated pits are large and prominent.

There are no light refracting organs.

The mouth is medio-ventral in position and is located near the anterior end. Its marginal contour is usually circular when at rest.

The highly muscular pharynx is lined by a ciliated epithelium. In the pharyngeal wall brownish longitudinal fiber-masses are very prominent. Pharyngeal glands, somewhat irregular in shape, may be seen lying outside the posterior half of the pharyngeal wall. These glands may also occur on the anterior half but are less numerous and less obvious here. The pharynx undercuts the antero-dorsal enteric region and opens into the antero-ventral wall of the enteron.

The cells of the enteric epithelium are not very distinct in outline and their free ends are finely ciliated. The outer margin of the enteric wall is faintly and irregularly dentate. The enteric epithelium is usually at least half again as thick as the epidermal epithelium and when the animal is extended the enteric lumen is, as a rule, small. Dark granular masses, isolated granules and vacuoles of various sizes are common in the enteric wall.

In the general parenchyma of the enteric region may be seen scattered cells of various sizes and shapes. In the pharyngeal and cephalic regions large spherical cells are rather plentiful.

The protonephridium passes from the extreme anterior end under the cephalic connective and thence over the pharynx to its posterior end where it bends sharply to the left and passes to the ventral surface of the enteron after which it passes caudad, somewhat to the left of the mid-ventral line, by a slightly irregular path. The peculiar position of the tube suggested the specific name. The posterior opening of the protonephridium is subterminal. One case in which the protonephridium passed ventrally to the right has been observed but a dorsal tube posterior to the pharynx has never been observed.

This species cultures readily in wheat-grain infusions and large cultures have been maintained for almost 7 years. Asexual reproduction occurs in healthy animals about once in 24

hours. No sexual reproduction has ever occurred in our cultures.

Specific diagnosis: Light-refracting bodies absent; a protonephridial tube which lies ventral to the enteron posterior to the pharynx; vesicular epidermal bodies.

S. virginianum Nuttycombe (Pl. V, Fig. 3)

Nuttycombe, 1931; Jones, 1932; Nuttycombe and Waters, 1935*c*.

S. virginianum exists in at least three varieties which differ primarily in size. Nuttycombe (1931) poorly described *S. virginianum* from Giles County, Virginia and Jones (1932) described the large variety as *S. carnivorum* from the University of Virginia and Halifax, Nova Scotia. We have collected an intermediate form from Stone Mountain, Georgia, and a large variety used by Jones from several localities in Georgia, Virginia and North Carolina. Van Cleave (1929), we believe, used this large variety under the tentative name *S. leucops*. Van Cleave's collections were made at Chicago, Illinois.

We have for several years maintained cultures of these varieties and have attempted to find some constant feature for differential diagnosis but no such features have been found.

Nuttycombe in his description of *S. virginianum* stated that pharyngeal "glands are not apparent but the nuclei of the muscle fibers in the pharyngeal region stand out sharply." Later studies of this species (specimens from the same clone as those from which original description was drawn) have revealed small glands scattered over the whole pharyngeal region with a somewhat greater concentration near the mouth. These glands correspond both as to size and distribution to those described by Jones for *S. carnivorum*. Jones' description of the refractive bodies of *S. carnivorum* leaves much to be desired. He states, "associated with the posterior lobe of each cephalic ganglion is a colorless vesicle of 'eye spot' very much like that which Kepner and Carter described for *S. ciliatum*." According to Kepner and Carter (1931) each

refractive body in *S. ciliatum* consists of "a colorless vesicle that contains on its posterior floor, one, two or three more or less rod-shaped light refracting bodies." In our large form *S. virginianum* all the characteristics conform to Jones' description of *S. carnivorum* except that the refractive mass associated with the vesicle always appears to be composed of a unit (this unit may show some indentations on its edge) which appears rod-shaped only from the side. Jones' figure is that of a considerably compressed specimen and in such an animal the side view of the refractive masses would be obtained. In addition to the morphological similarity the feeding habits of *S. carnivorum* and all varieties of *S. virginianum* are similar (see latter part of description).

As Nuttycombe's *S. virginianum* has priority we are eliminating *S. carnivorum* from our list of valid species. A description of *S. virginianum* follows:

Single zooids in the small variety are about 0.40 mm. in length; single zooids of the large variety are about 1.0 mm. in length. The maximum observed length for zooid chains in the small variety is slightly over 1.0 mm.; in the large variety, 5.0 mm.

The body is cylindrical with its greatest diameter in the region of the pharynx. Anterior to the mouth the body tapers sharply—the posterior end of the body narrows gradually and ends bluntly rounded. The anterior end is extremely mobile and is constantly changing shape.

In reflected light the color is glistening white. In transmitted light the body, except for the enteron which varies in color dependent upon its food-content, is translucent.

The general, epidermal, ciliary coat is heavy and of uniform length. The cilia are slightly longer than the epidermal thickness. Long (about 3 times the length of the cilia of the general ciliary coat), semi-rigid cilia are sparsely distributed over the body but are more prominent at the anterior and posterior ends of the body and around the oral opening.

The numerous, minute, rod-shaped, epidermal rhabdites are disposed perpendicularly to the surface and are largely confined to the distal ends of the cells.

The two, small, hemispherical, ciliated pits are located laterally or lateroventrally.

The nervous system conforms to the general pattern for the genus.

A light refractive body is associated with each posterior ganglionic lobe. At the end of a stem-like outgrowth from the lobe is a very faintly granular vesicle bearing the lens-like (roughly triangular, sometimes with dentate edges) refractive unit. The refractive face is directed postero-mesially.

The mouth is approximately round in outline when at rest. It is highly mobile and distensible.

The pharynx when at rest is of fairly uniform diameter throughout. Its walls contain a heavy circular and longitudinal musculature. The extrinsic, radial musculature of the pharynx is heavy. The pharyngeal opening into the enteron is guarded by a strong sphincter. Small unicellular glands are sparsely distributed over the outer surface of the pharynx but are somewhat more numerous at the anterior end. The pharyngeal epithelium is very lightly ciliated.

The enteron extends almost to the end of the pseudocoel leaving very little free tail region. The outer enteric wall is faintly and irregularly dentate. The enteric lumen is finely ciliated. The general color of the enteron varies from yellowish to brown in transmitted light. The enteric epithelium is finely granular and rather numerous, prominent cells (gland cells) which contain masses of larger refractive granules also occur. These cells stand out very sharply in specimens in which the enteron is not laden with food.

In the very limited space between the enteron and the body wall may be seen free cells of various sizes and shapes. In the region anterior to the enteron large free cells are fairly numerous between the muscle fibers about the pharynx and around and in front of the cerebral ganglia.

The radial musculature supporting the protonephridium and enteron is well-developed.

The very prominent, protonephridial tube passes from beneath the transverse commissure posteriorly above the

pharynx and enteron in a slightly undulating path to the posterior end where it opens subterminally.

All varieties of this species characteristically move about ventral side up.

According to Jones this species (his *S. carnivorum*) is primarily predatory upon other smaller species of *Stenostomum* which it ingests but it is incapable of "pinching" off pieces from the body surface of its prey after the manner of *S. predatorium*. It is certainly true that *S. virginianum* is incapable of feeding by the peculiar method of *S. predatorium* and it is further true that it frequently ingests whole individuals of other species or smaller individuals of its own kind but we do not consider this predatory feeding upon *Stenostomum* as primary. We have maintained pure cultures of this species without difficulty and except for occasional cannibalism the cultures have subsisted on protozoa and rotifers.

Specific diagnosis: 1 pair light-refracting bodies each composed of apparently 1 to 4 units; protonephridiopore subterminal; pharynx relatively long and tubular with small glands distributed over entire surface and with elaborate, radial pharyngeal musculature; little enteron-free caudal region; fairly prominent ciliary coat and long semi-rigid cilia sparsely distributed over body.

S. pseudoacetabulum Nuttycombe and Waters (Pl. III, Fig. 3)

Nuttycombe and Waters, 1935*a*.

This species was collected in large numbers from ponds in Bulloch County, Georgia.

Healthy specimens measure from 0.75 to 2.5 mm. in length. We have never encountered specimens without a fission plane and large individuals frequently show seven well-defined planes.

The head is sharply marked off by a constriction just anterior to the mouth and is somewhat spatulate. Posterior to the head the body is cylindrical and tapers gradually into an attenuated caudal region. The posterior end of this region bears a dorsally directed outgrowth very similar to that de-

scribed for *S. arevaloi*. In *S. pseudoacetabulum* however the outgrowth arises at the new fission plane as a postero-dorsal outgrowth from the anterior zooid. In well-fed individuals the body as a whole is very thick and heavy.

In reflected light the animal is white. In transmitted light the integument and anterior region are translucent or pale brownish. The enteric region is gray, brownish or almost black dependent upon food-content. The enteron has many glistening vacuoles.

The general ciliary coat is uniform—cilia about as long as epidermis (epidermis other than that in free caudal region; here it is thinner than elsewhere) is thick. Non-vibratile cilia, three or four times as long as those in the general coat, have been observed in the preoral region but not elsewhere.

Rod-shaped rhabdites, vertically disposed and uniformly distributed, lie just beneath the surface of the epidermal cells.

The two ciliated pits are of medium size and laterally placed.

The cephalic ganglia conform to the general pattern for the genus. Light-refracting bodies associated with the ganglia have not been observed.

From above the mouth at rest very strongly resembles a trematode acetabulum. This feature suggested the specific name. This sucker-like appearance is due to the fact that the "lips" are invaginated so deeply as to involve about the anterior one-third of the pharyngeal wall. This region may be everted rapidly to form a proboscis-like structure.

The pharynx is widened posteriorly to form a cap over the anterior end of the enteron. The pharyngeal walls are highly muscular and prominently glandular. A sphincter lies between the pharynx and the enteron. The pharyngeal lining is ciliated. The radial muscle fibers from the pharyngeal wall to the body are very numerous.

The enteron conforms in shape to the body region in which it lies. It terminates considerably anterior to the caudal end of the animal and leaves an enteron-free caudal region. The enteric epithelium is approximately twice as thick as that of

the epidermis. Its cells are prominently vacuolate and finely ciliated. Occasional granular "gland cells" occur.

The free cells of the pseudocoel are of various sizes and shapes.

The protonephridium passes posteriorly from the preganglionic region beneath the transverse commissure of the cephalic ganglia, dorsal to the pharynx and enteron, and empties terminally beneath the dorsal outgrowth at the posterior end of the body.

About half the specimens studied were sexually mature as males. The testis is located above and slightly to the right side of the pharynx.

Specific diagnosis: Light-refracting bodies absent; protonephridiopore terminal; single dorsally directed caudal outgrowth.

S. cryptops Nuttycombe and Waters (Pl. VI, Fig. 2)

Nuttycombe and Waters, 1935*b*.

This species has been collected in considerable numbers from ponds in Bulloch County, Georgia.

The maximum observed length for *S. cryptops* is 0.85 mm. for a chain of 5 zooids. The minimum for a single zooid 0.25 mm. The ratio of diameter to length ranges from 1 to 8 to 1 to 15.

In reflected light the animals are white. In transmitted light the epidermis, pseudocoel and pre-enteric region are translucent; the enteron varies in color dependent upon its food-content.

The general ciliary coat of the epidermal epithelium is uniform. The length of these cilia generally only slightly exceeds the thickness of the epidermis but occasionally the cilia are twice as long as the epidermis is thick. Sparsely scattered over the body are semi-rigid cilia 3 or 4 times as long as those of the general ciliary coat. The epidermal rhabdites are fairly large, rod-shaped and evenly distributed.

The cephalic ganglia conform to the general pattern of the genus but are extremely large and much elongated.

The ciliated pits are dorso-laterally placed, large and much deeper at their posterior ends.

There are two pairs of inconspicuous light refracting bodies of the same general type. The inconspicuousness of these bodies suggested the specific name. Both pairs have associated with them large, clear vesicles. The anterior pair lies at the posterior tips of the anterior lobes of the cerebral ganglia and the posterior pair lies, at rest, beneath the posterior lobes. The refractive mass associated with each of the vesicles of the anterior pair of light refracting bodies is smaller and less dish-shaped than the corresponding mass for the posterior pair of refractive bodies. The refractive masses of both pairs appear to be finely granular at a magnification of 1500. The connections of the anterior bodies have not been determined; the posterior pair appears to be associated with the posterior lobes. The posterior pair has a considerable range of lateral movement and may move mesially until they lie very close to each side of the mouth or laterally until they lie outside the lateral margins of the posterior lobes. They are shown in this latter position in the figure for this species.

The mouth is medio-ventral in position and is typically a longitudinal slit the posterior margin of which is located about the middle of the pharyngeal region.

The pharynx is short, highly muscled and has associated with its posterior end several long-necked glands of unusually large size. Smaller glands also occur more anteriorly in the pharyngeal region. The pharyngeal lining is ciliated. The pharynx opens posteriorly into the enteron through a strong sphincter muscle.

The antero-dorsal wall of the enteron overhangs the pharynx. Posteriorly the enteron tapers and terminates just anterior to the posterior end of the body leaving only a very short, enteron-free caudal region. The enteric epithelium is generally about twice as thick as the epidermis. The free ends of the enteric epithelial cells are finely ciliated. Lying within the epithelium are fairly numerous, granular "gland cells." The usual vacuoles and fine granules are also present.

The free cells of the pseudocoel are few, of various sizes, and are frequently highly refractive. The radial musculature between the body wall and internal parts is not as well developed as is usual in the genus.

The protonephridium is a delicate tube which passes from beneath the cerebral commissure posteriorly above the pharynx and enteron by a tortuous path to its subterminal opening.

Only asexual reproduction has been observed. This species cultures readily in wheat-grain infusions containing *Chilomonas* and other small protozoans.

Specific diagnosis: Two pairs of light-refracting bodies, an anterior pair lying at the posterior end of the anterior lobes of the cerebral ganglia and a posterior pair lying beneath the posterior lobes; protonephridiopore subterminal; mouth typically a slit the posterior margin of which lies beneath the middle of the pharynx; unusually large "brain"; very extensive preenteric region.

S. kepneri n. sp. (Pl. VII, Fig. 3)

The specific name *kepneri* is given because of the great esteem which the senior author holds for Dr. William A. Kepner of the University of Virginia.

This species has been collected from two ponds in the vicinity of Athens, Georgia, and from several localities in the region of Mountain Lake, Virginia.

The mid-region of the body is roughly cylindrical. The anterior end tapers abruptly and the posterior end narrows less abruptly to a bluntly rounded tip in front of which there is no extensive enteron-free caudal region.

The maximum observed length was 1.75 mm. We have never seen specimens of more than 4 zooids.

In reflected light the animal is white. In transmitted light the epidermis and anterior region are pale brown to translucent; the enteric region yellowish to almost black.

The uniform general ciliary coat is composed of cilia about as long as the epidermal thickness. No long cilia have been

observed. The small, rod-shaped, epidermal rhabdites are largely confined to the distal ends of the cells.

The cephalic ganglia are similar to those of *S. tenuicaudatum*.

The two rather large ciliated pits are laterally placed.

A pair of conspicuous, spherical light-refracting bodies are associated with the posterior ganglionic lobes. Each spherical unit is very closely associated with a mass projecting from beneath the latero-posterior face of the posterior ganglionic lobe. The vesicle of the body is to all appearances very closely applied to the projecting mass. We have noted in newly separated individuals and in the heads of well-advanced zooids that the refractive unit may be composed of many small granules. This fact suggests to us that the granular type of unit may be the more primitive type and the spherical or lens-like units may be derivatives of the granular type.

The mouth is medioventrally placed and is almost circular in outline when the "lips" are at rest.

The rather short pharynx has a well-muscled wall and a wide lumen. Externally the pharynx is covered by gland cells. Those over its anterior surface are relatively small whereas those over the posterior surface are very large and the bodies of the most caudal of them extend over the enteric wall. Posteriorly the pharynx opens into the enteron through an unusually prominent sphincter.

The outer enteric wall is irregularly dentate. The enteric lumen is wide. The enteric epithelium is about twice as thick as the epidermal epithelium and its cells are ciliated at their free ends. Within the enteric epithelium may be seen fairly numerous "gland cells" containing masses of fine granules. The other enteric cells show the usual vacuoles and dispersed granules. The enteron tapers posteriorly somewhat more abruptly than the corresponding body wall and terminates a short distance in front of the bluntly rounded body wall.

The free cells of the pseudocoel are rather numerous and the radial muscle fibers between the body wall and the posterior end of the enteron are large and numerous.

The prominent protonephridial tube passes from beneath the cerebral commissure above the pharynx and enteron by a tortuous path to the posterior enteric region. Somewhat anterior to the caudal tip of the enteron it bends abruptly to the right (occasionally to the left) and passes ventrally to open beneath the enteron.

This species cultures readily in wheat-grain infusions.

Only asexual reproduction has been observed.

Specific diagnosis: 1 pair of light-refracting bodies, each generally a single sphere; a unique feature is a protonephridiopore located beneath the posterior end of the enteron; pharyngeal glands large; ciliated pits large; little enteron-free caudal region.

S. anops n. sp. (Pl. VII, Fig. 1)

This species was collected from a swamp about 3 miles northeast of Athens, Georgia.

The thin body is cylindrical except for tapering at the anterior and posterior ends, both of which end in a bluntly rounded termination. There is practically no enteron-free caudal region. The region anterior to the mouth is somewhat flattened dorso-ventrally and is extremely mobile. Two or more constrictions are more or less constant features in the region between the anterior pharyngeal level and the ciliated pits. These constrictions are not fixed but result from almost constant activity of the musculature of the body wall in this region.

The maximum observed length was 1.25 mm. Specimens of more than 4 zooids have not been seen.

In reflected light the animal is white. In transmitted light the epidermis and anterior end of the body are translucent to yellowish; the enteron is gray, brown or almost black.

The uniform, general ciliary coat is composed of cilia which are slightly longer than the epidermal thickness. The epidermal rhabdites are extremely small and are located in the distal ends of the cells. The epidermis contains "rosette" gland cells very similar to those of *S. glandulosum*. In *S.*

anops, however, these bodies are less numerous and are not so prominent as in *S. glandulosum*. The ventral caudal region of the animal is highly adhesive.

The cephalic ganglia conform to the generic pattern and bear a general resemblance to those of *S. glandulosum*.

The dorso-lateral ciliated pits are relatively large. Additional pairs of ciliated pits such as described for *S. glandulosum* are not obvious.

Magnification of 1500 revealed no trace of light-refracting bodies.

The mouth is medio-ventral in position and exhibits a circular outline when adjacent parts are at rest.

The wall of the moderately long pharynx contains circular muscle bands of unusual prominence. Externally the pharynx is covered by large, long-necked gland cells.

The enteron passes almost to the posterior end of the body and ends bluntly rounded. The enteric epithelium typically contains numerous, prominent "gland cells" loaded with rather large granules. As a rule vacuoles are also rather prominent in the enteric epithelium.

The free cells of the pseudocoel are prominent only in the cephalic region.

The protonephridium passes from beneath the cerebral commissure posteriorly over the pharynx and enteron to its opening at the terminal tip of the body.

This species cultures very readily in wheat-grain infusions.

Only asexual reproduction has been observed.

Specific diagnosis: Light-refracting bodies absent; protonephridiopore terminal; "rosette" bodies in epidermis; little enteron-free caudal region.

S. tuberculosum n. sp. (Pl. I, Fig. 3)

This species has been collected in large numbers from the lake at Mountain Lake, Virginia and from a pond at Athens, Georgia.

The anterior end of the body is triangular from above with a slight constriction just back of the ciliated pits. Posteriorly

the body narrows gradually to the caudal end. The ventral surface is greatly flattened or, as is generally the case, appreciably concave. At the extreme tip of the cephalic triangle there projects a short tubercle which arises from a slightly broadened base just ventral to the extreme tip of the head. We have not been able to notice any marked structural adaptations associated with this tubercle nor have we ascertained any special function except that it seems to be extremely sensitive and may serve as a tactile organ. This tubercle suggested the specific name. There is usually a considerable enteron-free caudal region.

The maximum observed length was 0.8 mm. for a chain of 4 zooids (we have not seen a longer zooid chain than this) and the minimum length for a single zooid 0.2 mm.

In reflected light the animal is white and in transmitted light translucent or faintly yellowish except for the enteron which is yellowish, orange or greenish.

The uniform, dense general ciliary coat is composed of cilia the length of which is 3 to 4 times the epidermal thickness. Prominent, long, semi-rigid cilia, some of which are as long as the greatest diameter of the body, are sparsely scattered over the body. These long cilia appear much as those of *S. saliens*. The distal ends of the epidermal cells bear tiny rhabdites.

The cephalic ganglia fit well into the general pattern for the genus.

The two ciliated pits are dorso-lateral in position and are much wider at their posterior margins so that they appear to be directed anteriorly.

There are no light-refracting bodies.

The mouth is V-shaped and, viewed from the ventral side of the animal under low magnification, the refractive nature of the "lips" gives the appearance of containing a skeletal material. However, actual skeletal elements cannot be found. The powerfully muscled "lips" tend to operate laterally.

The ovoid pharynx is short, powerfully muscled and has associated with it very large unicellular glands. The anterior end of the enteron overlies the posterior end of the pharynx.

The outer enteric wall is prominently dentate. The enteric epithelium, which is about 4 times as thick as the epidermal epithelium, normally contains a few "gland" cells filled with very fine granules, many vacuoles and numerous fine scattered granules. The internal margins of the enteric cells are ciliated. The enteron does not usually extend to the extreme caudal end of the body.

The free cells of the pseudocoel are generally small and for the most part are highly granular.

The delicate protonephridial tube passes from beneath the transverse commissure posteriorly over the pharynx and enteron by a somewhat tortuous path and empties terminally.

This species feeds on active protozoa and is capable of darting forward with amazing rapidity for the capture of its prey. The rapidity of its movement even exceeds that of *S. saliens* which has the same prominent long cilia and feeding habits.

This species may be cultured with great ease in wheat-grain infusions.

Specific diagnosis: Light-refracting bodies absent; protonephridiopore terminal; mouth V-shaped; pharynx muscular, short, ovoid with large gland cells; ciliated pits prominent and anteriorly directed; very prominent, long, semi-rigid cilia sparsely scattered over body; a short tubercle projecting from the ventral margin of anterior tip.

S. pegephilum n. sp. (Pl. I, Fig. 1)

This species was collected in large numbers from 2 springs in the region of the Mountain Lake Biological Station, Mountain Lake, Virginia. Both of these springs, located at an elevation between 3700 and 3900 feet, are very cool (42° to 46° F.).

The body is typically a cylinder tapered gradually at the posterior end and more abruptly at the anterior end. There is little ventral flattening. The general body shape is somewhat more variable than for most species of the genus; some specimens appear rather plump whereas others are quite slender.

The anterior end is mobile and is capable of assuming a variety of shapes.

The maximum observed length was 0.9 mm. for 2 zooids (we have not encountered individuals of more than 2 zooids) and the minimum length 0.26 mm.

In reflected light the animal is white. In transmitted light the body, except for the enteron, is translucent; the enteron is yellowish, greenish or gray.

The epidermis is unusually thick. The general ciliary coat is uniform and is composed of cilia one and one-half to twice as long as the epidermal thickness. Sparsely scattered over the body are delicate, long cilia about twice as long as those of the general ciliary coat. The extremely tiny rhabdites are confined to the distal ends of the epidermal cells.

The cephalic ganglia are elongate and in general conform to the generic pattern.

The two ciliated pits are medium in size and are latero-ventral in position.

There are no light-refracting bodies.

The mouth is a trasversely disposed ovoid opening.

The pharynx is muscular and when extended is oval in shape. The posterior pharyngeal wall is frequently thrown into folds so as to give the rear of the pharynx the appearance of a separate chamber. The pharyngeal lining is very heavily ciliated. The outer surface of the pharynx is covered by unicellular glands of medium size. Posteriorly the pharynx opens into the enteron through a sphincter.

The enteron rarely extends to the extreme caudal end of the pseudocoel. The outer enteric wall is only faintly and irregularly dentate. The enteric epithelium has 2.5 to 3 times the thickness of the epidermis. The enteric lumen is wide. The inner surfaces of the enteric cells are finely ciliated. "Gland cells" containing masses of small granules are fairly numerous in the enteric epithelium.

A very evident feature in the pseudocoel of this species are numerous, highly refractive masses composed of granules of various sizes. There are also in the pseudocoel cells of the usual types.

The prominent protonephridial tube follows the conventional path over the pharynx and enteron to its pore at the terminal tip of the body.

This species apparently secretes much mucus and is given to forming cocoon-like structures of sediment particles. It is sometimes quite difficult to dislodge specimens from the sedimentary masses. The animals show little tendency to rise to the surface in an aquarium and have to be searched for in the sediment on the floor of the vessel.

This species does not culture readily in wheat-grain infusions but may be cultured easily in sediment containing protozoa and unicellular algae. It apparently feeds upon sediment and algae.

Only asexual reproduction has been observed.

Specific diagnosis: Light-refracting bodies absent; protonephridiopore terminal; mouth typically transverse, ovoid; pharynx covered by glands of medium size; ciliated pits medium size, ventro-lateral in position; no special epidermal bodies, rhabdites extremely tiny; usually prominent refractive masses in pseudocoel.

Poorly Described Species or Species of Questionable Validity

S. leucops Dugès

A. Dugès, 1828; O. Schmidt, 1848; Graff, 1882; Ott, 1892; Graff, 1904; Graff, 1913. See Graff 1913 for synonymy of this species; for other papers see abstracts in Bronn's Das Tierreich. Turbellaria II. Rhabdocoelida.

The most perplexing problem which we have encountered concerns the validity of *S. leucops* as an American species. The literature is replete with references to *S. leucops*. For instance, Ott (1892) made an extensive biological study of this species, Child (1901, etc.) used it for a study of fission and regulation and later for comparative studies in reconstruction. Kepner and Cash (1915) described the ciliated pits of a species they supposed to be *S. leucops* and Van Cleave (1929) used a species which he tentatively identified as *S. leucops* for studies in fission and reconstitution. Van Cleave (1929) states that

Child "now believes his identification of the species he then called *S. leucops* to have been incorrect and that the form was probably *S. tenuicauda*." Kepner and Carter (1931) state that they believe Kepner and Cash described the ciliated pits of *S. tenuicaudatum* instead of *S. leucops*. Kepner further states that, if the original description of *S. leucops* be correct, he has never found *S. leucops* in Virginia. We are convinced from his description, sketches and discussion of the reconstitutional behavior that Van Cleave (1929) used *S. virginianum* instead of *S. leucops*.

Ott's lengthy description of the species still leaves the status of *S. leucops* very uncertain. He describes light-refracting bodies which might well be those of *S. tenuicaudatum* except for the fact that he does not mention the vesicle. He states that they are connected to the posterior brain lobes by a thin nerve. We are very dubious about any light-refracting body without an associated vesicle, even though *S. simplex* has been so described, particularly if it be of the dish-shaped type. Ott described the protonephridiopore as lying sometimes in the middle of the triangular, enteron-free, tail region and sometimes near the end of that region. The latter condition, as he points out, would obtain in newly divided anterior zooids. In the "short-tailed" variety of *S. tenuicaudatum* identical conditions are found with reference to the protonephridiopore. Ott's description of the pharynx and other parts of *S. leucops* conforms perfectly to the description of *S. tenuicaudatum*. He fails to mention the "enteric gland cells" which von Graff (1911) figures so prominently in his *S. tenuicaudatum* but we have seen *S. tenuicaudatum* without these. Indeed we feel almost certain that *S. leucops* according to Ott is the same as von Graff's *S. tenuicaudatum*.

Von Graff (1913) gave a description of *S. leucops* which appears to us, after studying the sources, to be a composite from the descriptions from Ott, Child and others. Child, as has been previously stated, now believes he used *S. tenuicaudatum* instead of *S. leucops* and it has been shown above that there is little of Ott's description which cannot be applied

to *S. tenuicaudatum*. Von Graff (1913), in his description, suggests two features which surely do not fit *S. tenuicaudatum*. According to him the pharynx of *S. leucops* is not uniform in structure but has a sort of oesophageal portion. Further, he describes a rhabdite-free tract down the middle of the ventral surface. Both of these features are found in *S. grande*. The opening of the nephridiopore, according to Ott, is located near the end of, or in the middle of, the tail "triangle." According to von Graff the opening is terminal or subterminal. We interpret terminal to mean extreme tip and subterminal to mean just anterior to the tip. We do not interpret "subterminal" as corresponding to "in the middle of the tail triangle." In short, there is no agreement as to specific characteristics for *S. leucops*. It seems evident that some variety of *S. tenuicaudatum* has been used in the United States for *S. leucops*. Furthermore, von Graff's description, which is the most extensive except for Ott's, seems to be a useless composite involving features drawn from several sources of questionable validity.

It might seem reasonable to reject the name *S. tenuicaudatum* and accept the name *S. leucops* as the valid specific name except for the all-important fact that von Graff's original brief description (1911) of *S. tenuicaudatum* fits the species which we are using under that name and Ott's extensive description of his *S. leucops*, with the exception of his questionable interpretation of the light-refracting bodies, also fits *S. tenuicaudatum*. Von Graff (1911), without descriptive text except for a comment on the length of zooids and rhabdites, records the collection of *S. leucops* at Rochester and Ontario. This and other collection records are open to the same criticism as those given Ott's *S. leucops*.

We are therefore placing this species in our second list as a highly questionable species. Indeed it seems likely to us that the descriptions of *S. leucops* are so unreliable as to invalidate completely that species in any fauna; we rather strongly suspect that it is a synonym for *Stenostomum*.

S. agile Silliman

Silliman, 1885; Graff, 1911; Graff, 1913.

Silliman collected this species in Monroe County, New York; von Graff states that it is abundant in the environs of Rochester, New York, and that he also found it in Switzerland and Bohemia. It has been reported from Europe by Fuhrmann (1894), Meixner (1915), Nasonov (1926), and others and by Okugawa (1930) from Japan.

We, with considerable misgiving, have placed *S. agile* in this group of questionable species. We have carefully studied and compared the original description by Silliman (1885) and those of von Graff (1911, 1913). The most significant specific feature appears to us to be the light-refracting body. This feature, as we have shown, is extremely constant within rather narrow limits and is reliable as a specific character. The light-refracting body in *S. agile* is unique in the American species if it is as described by von Graff. The original and von Graff's descriptions differ in several points. Silliman (1885) describes the light-refracting body as a sac which bears in its posterior part a strong light-refracting body which appears homogeneous. His figure shows an elevation on the anterior, concave face of this refractive unit. According to von Graff (1911) the body is composed of a fluid-filled vesicle which bears on its posterior face a lens-like refractive unit (4 microns in diameter) which sometimes shows striations or wrinkles. Lying embedded in a pit in the anterior face of this refractive lens is a globule. Von Graff (1913) omits any reference to the striae or wrinkles of the refractive unit but again describes the globule lying in a centrally located depression in the anterior face of the refractive lens. A refractive body with a globule lying in the anterior face of its refractive unit would unquestionably establish this as a valid species.

Silliman describes pharyngeal glands as being confined to the posterior end of the pharynx, whereas von Graff (1911, 1913) described and figured the glands over the entire pharynx.

Silliman described the ciliated pits as supplied by nerves from the anterior ganglionic lobes but Graff (1911) stresses the fact that the pits lie embedded in the anterior ganglia.

The three descriptions, Silliman (1885), von Graff (1911, 1913) agree in most other features. A composite of these follows.

Solitary zooids measure about 0.75 mm. in length; 5-zooid chains about 4 mm. Both ends of the body are tapered but blunt. The extensive anterior end contains elaborate transverse "muscle plates." The animal is colorless or occasionally gray or yellowish. There is a well-developed, epidermal, ciliary coat. The rhabdites are 2–4 microns in length. The posterior end of the body bears adhesive cells which allow firm attachment. The brain conforms to the generic pattern. The ciliated pits are far anterior and extra long cilia are associated with them. The light-refracting bodies have been previously described. The mouth lies in the anterior 1/5 of the body. The pharynx is long and covered by glands (see previous discussion of this feature). The intestinal epithelium is ciliated and contains glistening oil droplets and yellowish-green granules. The excretory system is similar to that of the generic pattern. The protonephridiopore lies somewhat anterior to the posterior end. The pseudocoel contains nucleate cells. Silliman figures and describes a saccular testis above the anterior pharyngeal region.

S. incaudatum Sonneborn

Sonneborn, 1930 *a* and *b*.

Sonneborn originally isolated a single specimen of this species from a culture in the Zoological Laboratories at Johns Hopkins University. The exact source of the culture could not be ascertained.

We think it very probable that this is a valid species but because of the nature of the description we are not placing it with our list of valid species. Sonneborn states that members of this genus have "one or more pairs of light-refractive organs of supposed optic function" and then fails to mention even the presence or absence of such organs in his description of *S. incaudatum*. His figures are of little value in determining the validity of his species.

His description is reproduced here:

"*Stenostomum incaudatum* differs from all other species of the genus in its combination of two characteristics; the absence of a tail, and a particular form and structure of the pharynx. The posterior end of the body tapers but slightly and ends bluntly; the pharynx lacks glands, is short, and is nearly as wide as the head. A full description follows:

"The body forms a flattened cylinder of nearly uniform depth and width except near the anterior and posterior ends. The ratio of width to length is about one to twenty in the longest chains, about one to five or six in solitary animals. The ratio of width to depth is about seven to five. Chains of as many as nine zooids occur. Single animals may be as long as 0.3 mm.; chains of nine zooids attain a length of 2.0 mm.

"In reflected light the whole organism appears white. In transmitted light the head is gray, the trunk dark. The darkness of the trunk is due chiefly to the food within the lumen of the intestine and within the intestinal cells, but even starved specimens have a yellowish brown intestine. Numerous dark green bodies (glands) are applied closely to the outer surface of the intestine; these can be seen more clearly in starved specimens.

"The form and structure of the head are represented in figure 3. In the most anterior part are two groups of transverse muscles. Just posterior to these lie the pair of shallow ciliated pits; at the base of each is a ganglion. These ganglia are immediately anterior to a pair of large, trapezoidal cerebral ganglia. No commissure between the cerebral ganglia has been observed, but these lie very close together. The most posterior part of the head is fully occupied by a thick-walled, muscular, but non-glandular, pharynx. A medioventral V-shaped mouth opens into the pharynx and the pharynx opens through a narrow sphincter into the intestine. The cavity of the pharynx is flask-shaped, with a narrow neck of the flask leading from the mouth to the wide base of the flask near the intestine. The base of the flask is partly occupied

by a pyramidal mass of cells with the sphincter in the center. After a food organism is captured, it is whirled around in the pharyngeal cavity for a short time and then pushed back into the intestine through the sphincter.

"The postpharyngeal region presents few unusual features. The pseudocoel is very narrow. Dark green glands, filled with granules or vacuoles, lie closely applied to the outer surface of the intestine. The distal parts of the intestinal cells are clear and translucent, sometimes containing translucent globules; the proximal parts are dark. The ciliated lumen of the intestine is wide. The excretory, or protonephridial, duct has the usual form and position. The ciliated integument is thin and translucent.

"No tail is present; the intestine extends to the posterior tip of the trunk."

He cultured this species easily on *Colpidium striatum*.

Species of Very Doubtful Validity

S. giganteum Higley

Higley, 1918.

Higley described this species from Illinois.

We have no apology to offer in classing this species as questionable. Both the description and the figure accompanying it are too inadequate to be worth anything. The whole description suggests one which might be offered for any of the larger species in a very unhealthy condition. Neither light-refracting bodies nor pharyngeal glands are mentioned and, indeed, the six pages devoted to the description of this species give little idea of its morphology other than its size and shape.

S. glandiferum Higley

Higley, 1918.

Higley collected this species in Illinois.

The description of this species is somewhat better than that of *S. giganteum* but is wholly inadequate to establish the species. We see some similarity between this species and

S. virginianum but we do not feel that the resemblance is sufficient to jeopardize the position of *S. virginianum*.

S. speciosum Stringer

Stringer, 1918 in Ward and Whipple's Fresh-water Biology, 1918.

The only descriptive material we have been able to locate on this species is found in Ward and Whipple, "Fresh-water Biology," 1918. The specimen figured is sexually mature as a female and the description is so brief as to be absolutely worthless.

Described from Nebraska.

Notes on Distribution, Collection, Culture, and Reproduction

There is little space in a paper of this length for discussion of the general biology of the genus. We have therefore limited this section to a few very brief notes on distribution, collection, culture methods, and reproduction.

Species of this genus appear to be widely distributed. The literature of the American species shows collection records from practically all parts of the country east of the Mississippi. Our own collection records show the genus plentiful in New York, Pennsylvania, Virginia, North and South Carolina, Tennessee, and Georgia. Foreign collection records (those for the American species are listed elsewhere in this paper) report the genus from all parts of the world.

We have found a variety of species at an elevation of about 5000 feet and an even greater variety at an elevation only a few feet above sea level. Our collection records indicate that *S. tenuicaudatum* and *S. virginianum* have the widest distribution and are found in greater numbers than other species. *S. grande*, *S. predatorium*, *S. saliens*, *S. arevaloi*, and *S. glandulosum* are also very widely distributed. *S. uronephrium*, *S. ventronephrium*, *S. pseudoacetabulum*, *S. cryptops* and others have only been collected in restricted territories.

Stenostomum are easily obtained from springs, ponds, lakes, slowly running streams or swamps by simply collecting a thin layer (about 1/2 inch) from the floor of the water bed or by collecting sediment-covered aquatic vegetation. We have made satisfactory collections at the very edge where the water was scarcely an inch deep and at a depth of several feet. As a rule winter collections are more satisfactory at the deeper levels.

We usually place each batch of collected material in a small aquarium filled with water from the source of the collection. Within a few days the culture becomes somewhat stagnant and the *Stenostomum* rise to the surface where they may be easily isolated with a pipette. If the site of collection was well selected such a culture will usually yield numerous individuals of from 3 to 5 species.

Most species may be maintained in pure culture in wheat-grain infusions. Such infusions are prepared as follows: Place seed wheat in cool spring water, heat to a boil and allow to cool. Add 4 grains of this wheat to 250 cc. of spring water (previously boiled, cooled and aerated) in a culture dish and allow to stand for three or four days—long enough for bacterial growth to get well started—and then inoculate with *Stenostomum*, protozoa, etc. from the collection. We have maintained a clone of *S. tenuicaudatum*, by subculturing it about every six weeks, in such infusions for 11 years. Some species, however, do not culture readily by this method. We have found that *S. arevaloi*, *S. glandulosum* and *S. predatorium* require other species of *Stenostomum* as food. We have been unable to culture satisfactorily *S. bicaudatum*. We have included with the specific descriptions notes on culture.

Reproduction is generally by fission and in most species is fairly rapid. We have had as many as 3 asexual generations in 24 hours from the small variety of *S. virginianum* and for the same variety we have had a specimen live three weeks without food during which time it did not divide and progressively became smaller. The larger species usually divide more slowly than the smaller species. Child (1902*a*) de-

scribed rather thoroughly the morphological changes associated with the process of fission.

Sexual reproduction has not been observed for all species. Sexuality generally occurs intermittently and, as has been pointed out by Sekera (1903*b*), Kepner, Carter and Hess (1933) and others, is apparently dependent upon environmental factors. In nature we have collected *S. tenuicaudatum* sexually mature as females in the fall of the year and Child (1901) and others have reported sexuality in specimens collected at that time of year. Kepner, Carter and Hess report the collection of *S. oesophagium* (variety of *S. grande*) sexually mature as males during the last week of August and during September but not during other months. They maintained cultures of *S. oesophagium* always sexually mature as males and intermittently as females. We have maintained cultures of this species (*S. grande*) for 4 years always sexually mature as males but no female gonads have appeared. On the other hand, we have maintained *S. tenuicaudatum* in wheat-grain infusions for 11 years without the appearance of any sexuality during which time more than 1000 asexual generations have passed.

The single, lobulated testis lies in the antero-dorsal pharyngeal region and opens dorsally by a pore. The ova are formed and supposedly undergo their early development in the pseudocoel. There are no ducts associated with the female gonads. Keller (1894), Sabussow (1897), Sekera (1903*b*, 1906), Kepner, Carter and Hess (1933), and others have contributed most of the information concerning sexual reproduction in *Stenostomum*. Probably the best discussion of sexuality in *Stenostomum* is that of Kepner, Carter and Hess on sexuality in *S. oesophagium*.

For general information on the genus we would recommend such papers as those of Graff (1882), Graff in Bronn's Klassen und Ordnungen des Tierreichs (1904–1908) and the later abstract numbers of Bronn's, Graff (1913) and Bresslau (1933). We believe our bibliographic files are fairly complete on this genus up to the present date and we will be glad to

supply any special information from these files to those who may desire it.

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LABEL ABBREVIATIONS FOR PLATES

cg, cerebral ganglion
co, caudal outgrowth
cp, ciliated pits
do, dorsal caudal outgrowth
e, posterior division of pharynx
ec, epidermic cross-section
en, enteron
ep, surface view of epidermis
ep$_1$, surface of ventral epidermis
ep$_2$, surface of dorsal epidermis
es, epidermal body
ese, epidermal gland
ev, enteric vacuole
f, free cells of pseudocoel
g, intestinal gland cell
gg, gland of grasping organ
gi, gizzard
go, tentacle-bearing "grasping organ" of pharynx
l, lip
le, long cilia
m, mouth
md, mandible-like lip
o, ovary
ph, pharynx
ph.g, pharyngeal glands
pn, protonephridium
r, radial muscle fiber
rb, light refracting body
s, ventral view of epidermis showing rhabdite free tracts
t, testis
tb, cephalic tubercle
v, vesicle of light refracting body
vo, ventral caudal outgrowth

PLATE I

FIG. 1. Dorsal view of *Stenostomum pegephilum*. *A*, outline of anterior end to show a characteristic shape assumed; *B*, outline of median dorso-ventral section of anterior end; *C*, chain of 2 zooids, *D*, section of epidermis to show rhabdites, general ciliary coat and large semi-rigid cilia.

FIG. 2.* Ventral view of *Stenostomum membranosum*.

FIG. 3. Ventral view of *Stenostomum tuberculosum*. *A*, chain of 4 zooids numbered to indicate relative age of fission planes; *B*, trans-section through mid region to show ventral concavity; *C*, outline lateral view of anterior end to show position of tubercle.

* Redrawn from Kepner and Carter (1931).

PLATE I

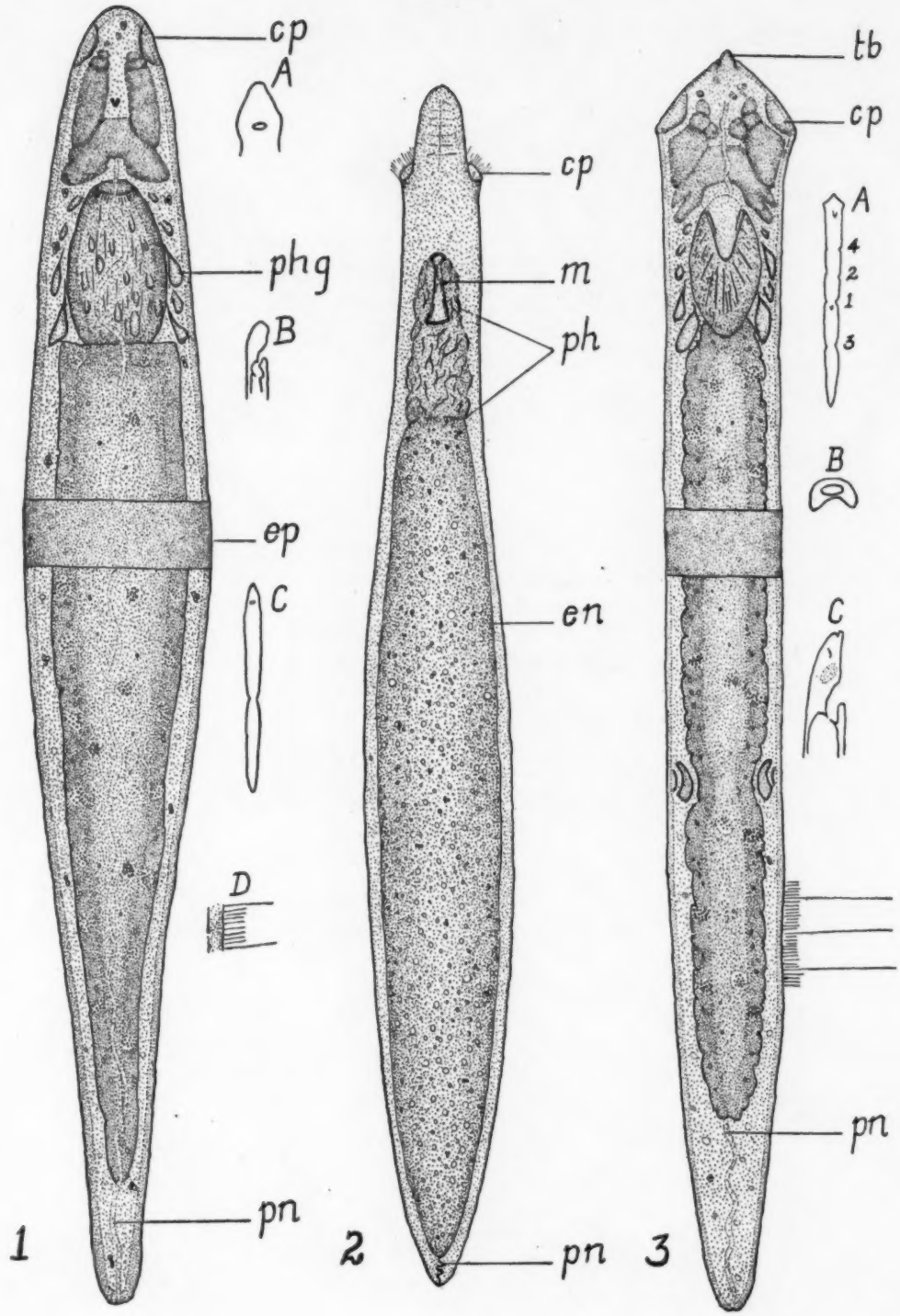

PLATE II

FIG. 1.* Dorsal view of *Stenostomum tauricum*.
FIG. 2.* Ventral view of *Stenostomum brevipharyngium*.
FIG. 3.* Dorsal view of *Stenostomum simplex*.

* Redrawn from Kepner and Carter (1931).

PLATE II

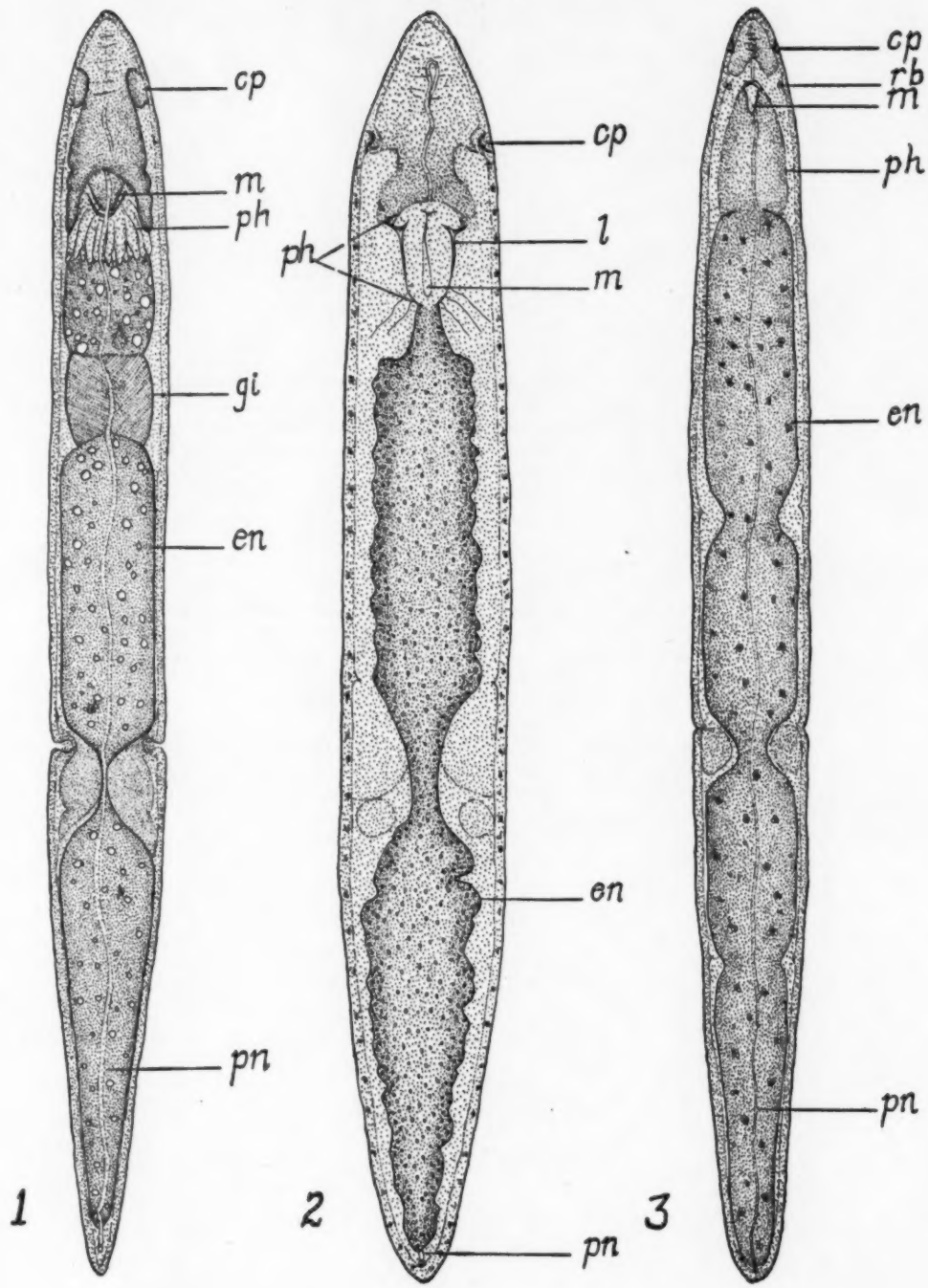

Plate III

Fig. 1. Dorsal view of *Stenostomum glandulosum*. *A*, lateral view of anterior region showing shape of pharynx; *B*, chain of six zooids with numbers indicating relative age of fission planes; *C*, section through epidermis showing rosette-shaped gland cells and general ciliary coat; *D*, rosette-shaped gland cells; *E*, two unusual patterns of light refracting bodies.

Fig. 2. Dorsal view of *Stenostomum arevaloi*. *A*, lateral view to show position of developing caudal outgrowth at new fission plane; *B*, ventral view of developing caudal outgrowth at new fission plane; *C*, highly magnified enteric epithelium just back of pharynx to show heavy granules (cilia not shown); *D*, chain of eight zooids with numbers indicating relative age of fission planes; *E*, protonephridium with translucent inclusion; *F*, highly magnified epidermis showing rhabdites, epidermal body and general ciliary coat; *G*, highly magnified enteric epithelium from region about middle of animal (cilia not shown); *H*, unusual pattern of light refracting bodies; *J*, outline shape of epidermal body.

Fig. 3. Dorsal view of *Stenostomum pseudoacetabulum*. *B*, lateral, optical view of anterior end of animal to show protruded "lips"; *C*, lateral, optical view of anterior end of animal to show "lips" folded into oral cavity; *D*, lateral, optical view of fission plane showing formation of caudal outgrowth, mouth and pharyngeal region; *E*, section showing enteric epithelium, pseudocoel, radial musculature, epidermis with rhabdites and general ciliary coat; *F*, chain of eight zooids with numbers indicating relative age of fission planes; *G*, posterior end of animal showing caudal outgrowth and opening of protonephridium.

PLATE III

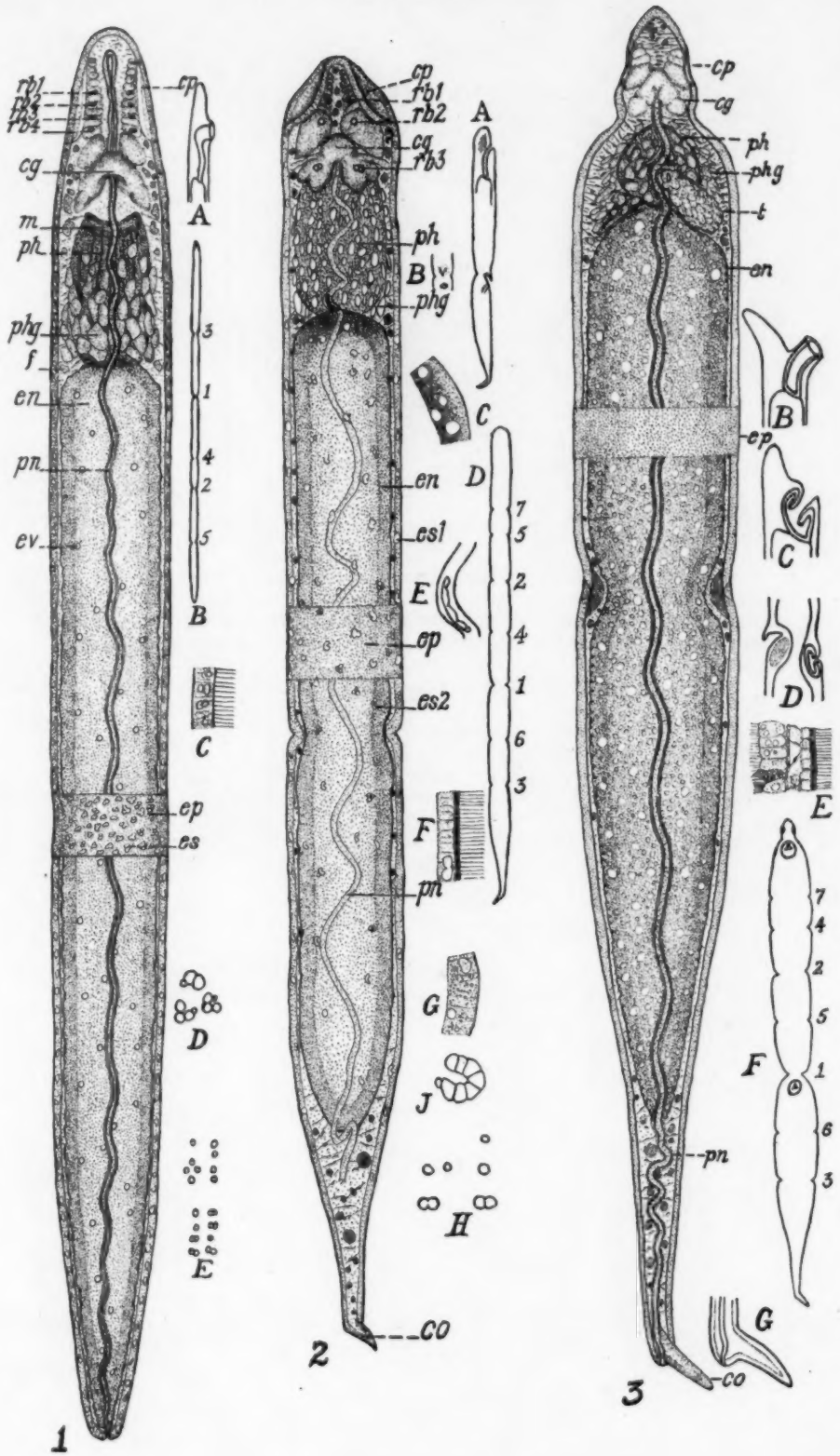
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Plate IV

Fig. 1. Dorsal view of *Stenostomum tenuicaudatum*. *A* and *B*, lateral and dorsal aspects of anterior region to show pharyngeal shape and relationships; *C*, section through epidermis showing rhabdites, general ciliary coat and a long semi-rigid cilium; *D*, chain of five zooids, with numbers indicating relative age of fission planes.

Fig. 2. Dorsal view of *Stenostomum uronephrium*. *A*, varieties of light refractive bodies.

Fig. 3. Dorsal view of *Stenostomum predatorium*. *A*, chain of nine zooids with numbers indicating relative age of fission planes; *B*, optical sagittal section of anterior end showing mouth and slight preoral pharyngeal region; *C*, outline of mouth at rest and open; *D*, epidermis showing general ciliary coat and semi-rigid cilia.

PLATE IV

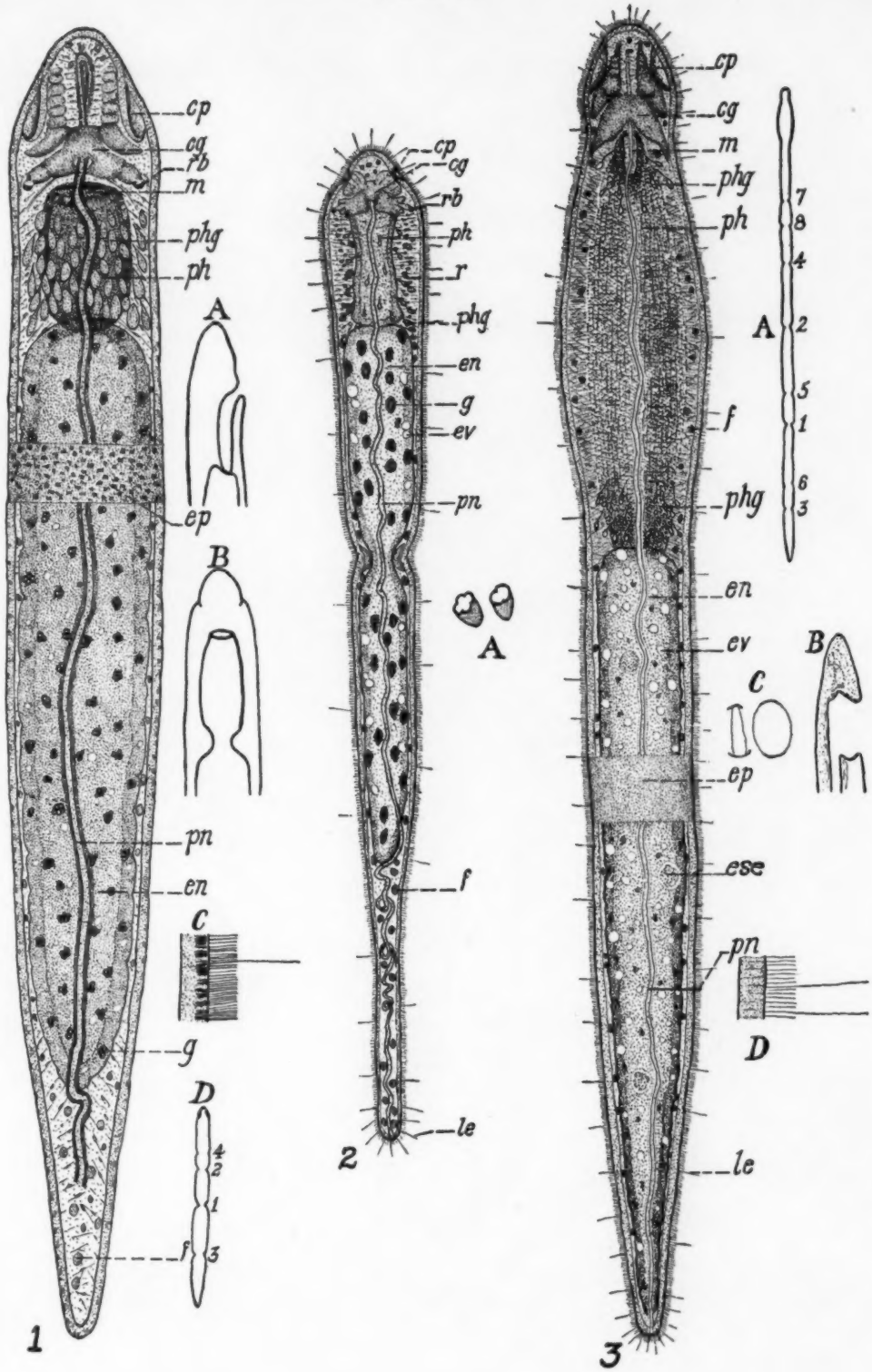
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PLATE V

FIG. 1. Dorsal view of *Stenostomum grande*. *A*, section of epidermis showing rhabdites, general ciliary coat and semi-rigid cilia; *B*, schematic cross section through animal showing epidermis with ventral rhabdite free tracts.

FIG. 2. Ventral view of *Stenostomum saliens*. *A*, diagram showing extremely long, stiff cilia; *B*, section through epidermis showing general ciliary coat.

FIG. 3. Dorsal view of *Stenostomum virginianum*. *A*, lateral view of anterior end showing mouth and pharynx; *B*, ventral view of anterior end; *C*, section showing enteric epithelium (cilia not shown), epidermis with rhabdites, general ciliary coat and long semi-rigid cilia; *D*, varieties of light refracting bodies; *E*, chain of eight zooids with numbers indicating relative age of fission planes.

PLATE V

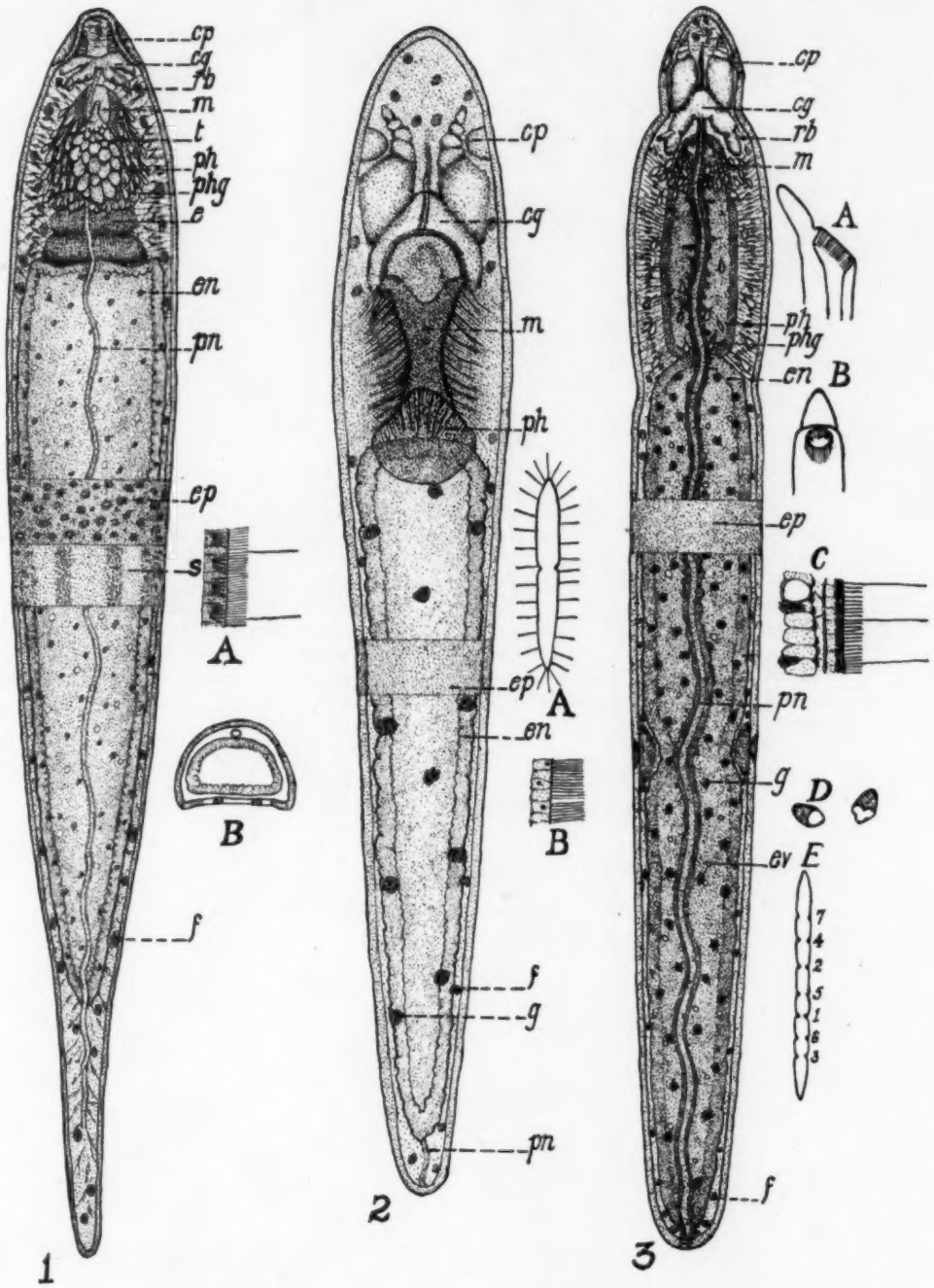
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PLATE VI

FIG. 1. Dorsal view of *Stenostomum ventronephrium*. *A*, section of epidermis at anterior end showing large rhabdites; *B*, section of enteric epithelium; *C*, chain of three zooids with numbers indicating relative age of fission planes; *D*, optical, sagittal section through posterior end showing protonephridiopore.

FIG. 2. Dorsal view of *Stenostomum cryptops*. *A*, optical sagittal section through anterior end showing mouth, pharynx, pharyngo-enteric sphincter and enteron; *B*, chain of four zooids with numbers indicating relative age of fission planes; *C*, section showing enteric epithelium (cilia not shown), pseudocoel, radial musculature, epidermis with rhabdites, general ciliary coat and long semi-rigid cilia.

PLATE VI

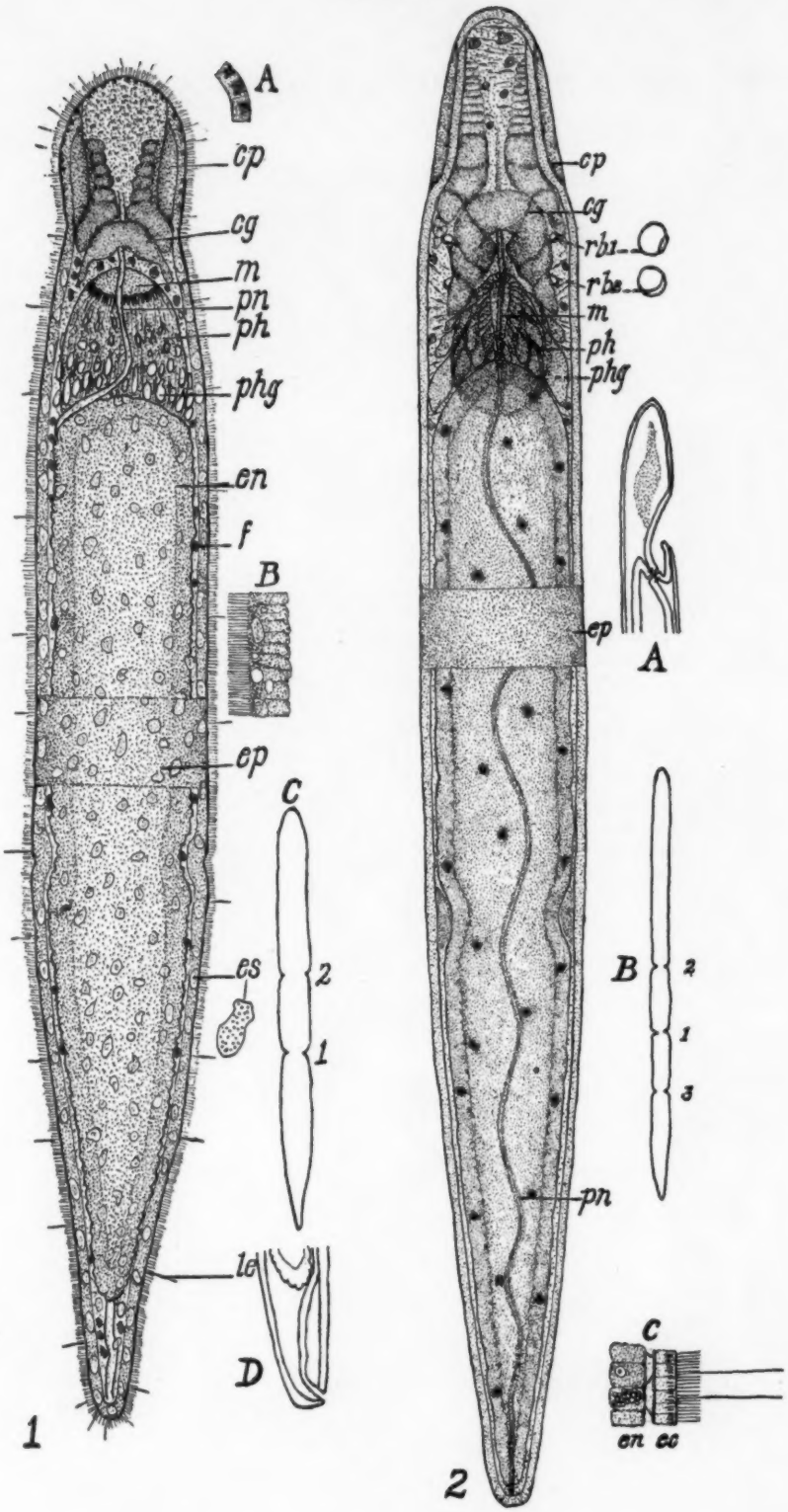
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PLATE VII

FIG. 1. Dorsal view of *Stenostomum anops*. *A*, optical section, in outline, of anterior end; *B*, chain of 4 zooids numbered to indicate relative age of fission planes; *C*, enlarged section of epidermis to show "rosette" body, rhabdites and cilia; *D*, enlarged enteric cells (cilia not shown).

FIG. 2. Ventral view of *Stenostomum bicaudatum*. *A*, outline of brain from dorsal side to show light refracting bodies of right side; *B*, outline of two-zooid specimen from side to show development of ventral caudal outgrowth at fission plane; *C*, section of epidermis to show epidermal bodies, rhabdites, cilia; *D*, two of tentacular organs, one showing the large inner cells.

FIG. 3. Dorsal view of *Stenostomum kepneri*. *A*, optical section, in outline, of an anterior end; *B*, chain of 3 zooids numbered to indicate relative age of fission planes; *C*, section of epidermis to show cilia and rhabdites; *D*, optical section of posterior end to show position of protonephridiopore.

PLATE VII

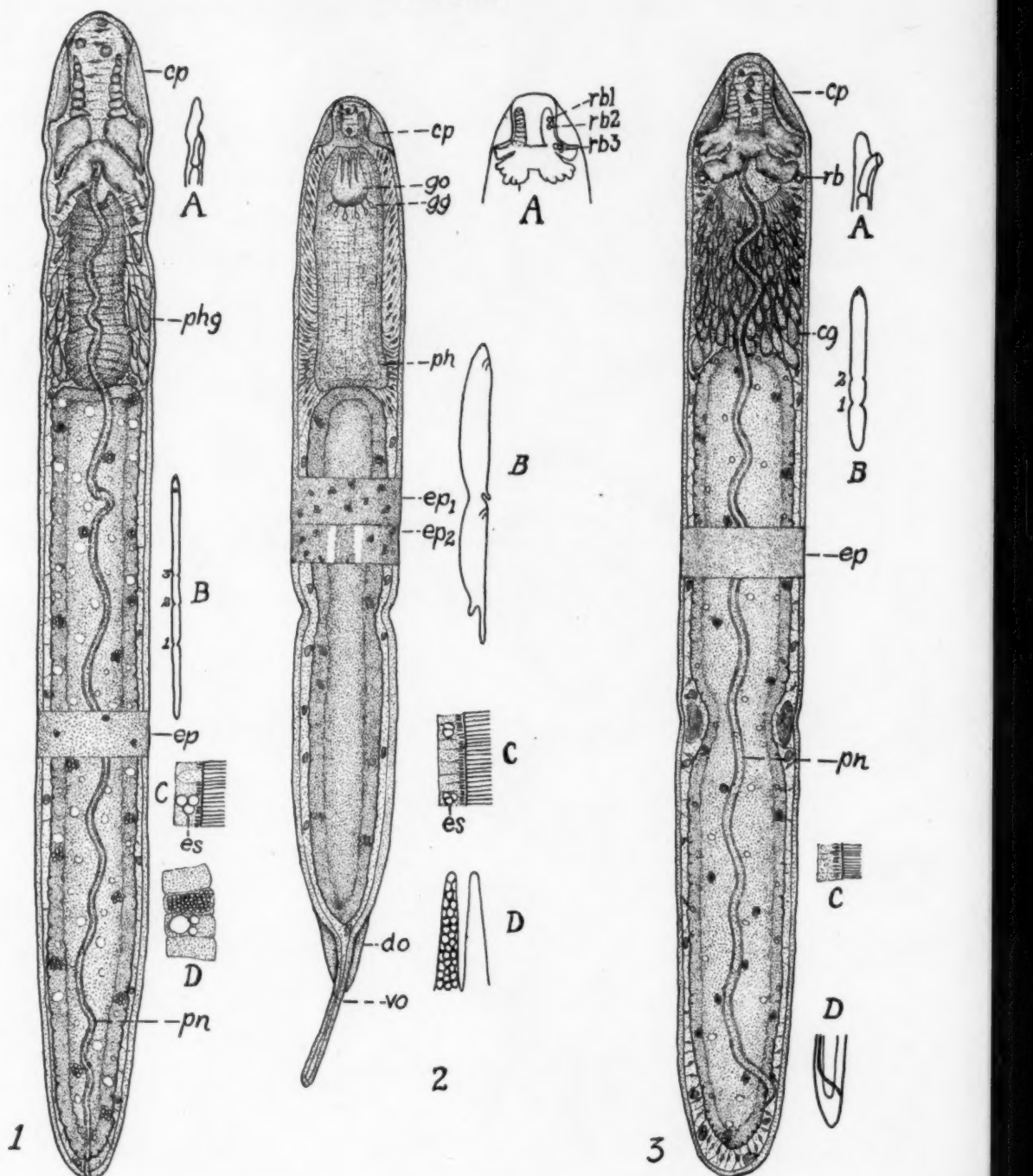
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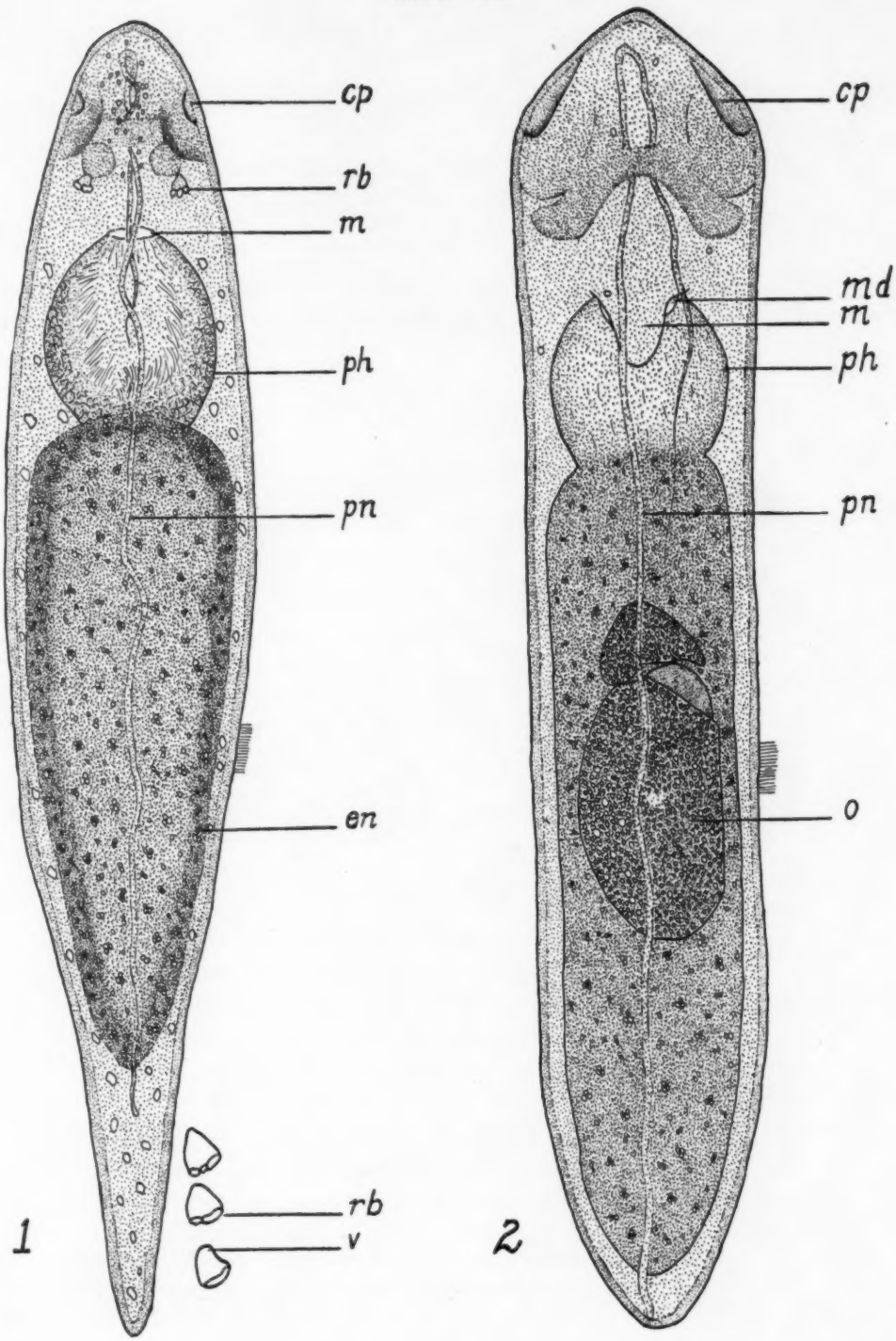
PLATE VIII

FIG. 1.* Dorsal view of *Stenostomum ciliatum*. *B*, varieties of light refracting bodies showing single, double and triple rod-shaped granules.

FIG. 2.* Dorsal view of *Stenostomum mandibulatum*.

* Redrawn from Kepner and Carter (1931).

PLATE VIII



ERRATUM

On page 303:

"Communicated by Albert F. Blakesley"

should read

"Communicated by Albert F. Blakeslee"

INFLUENCE OF THE GROWTH OF THE HOST ON OAT SMUT DEVELOPMENT[1]

GEORGE M. REED

Curator of Plant Pathology, Brooklyn Botanic Garden

(Communicated by Albert F. Blakesley)

ABSTRACT

The oat smuts penetrate the oat plant in the seedling stage, the mycelium continuing to develop for several weeks in intimate association with the embryonic host tissues. External factors, such as soil temperature and soil moisture, influence penetration.

This investigation is concerned with the question whether marked changes in the growth of the host from the seedling stage to maturity, involving the length of the growing period, the height and tillering of the plants, and the development of the panicles and spikelets, influence the final development of the smut organism.

Definite strains of oats, varying in their known reaction to specialized races of *Ustilago avenæ* (Pers.) Jens. and *U. levis* (K. & S.) Magn., were inoculated and grown under varied conditions of illumination and mineral nutrients supplied. Under artificial illumination, oats headed out several days to four weeks in advance of the non-illuminated controls, the plants being shorter, with fewer tillers.

Plants supplied with modifications of different nutrient solutions showed marked changes in the rate of growth, the extent of elongation of the stems, the amount of tillering, and also the time required from planting to the heading stage.

There seemed to be no definite relation between the growth of the host and the ultimate expression of the smut.

INTRODUCTION

THE intimate association between host and parasite furnishes fundamental problems for investigation. Studies have been made to establish, if possible, some definite connection between their relation and the absorption of mineral nutrients by the host. Profound changes in the growth of plants are induced by modifications of the nutrients supplied. The results are especially evident in experiments involving the elements nitrogen, phosphorus and potassium, although calcium, magnesium and iron produce striking results. In

[1] Supported in part by a grant from the Penrose Fund of the American Philosophical Society. Brooklyn Botanic Garden Contribution No. 84.

recent years, the important role of traces of copper, manganese, boron and zinc, has been studied.

The present investigation was carried out in order to determine whether changes in the rate of growth of the oat plant, the extent of elongation of the stems, the amount of tillering, the size of the panicles and the number of spikelets, influenced in any way the ultimate development of the oat smuts. Marked variations in plants were obtained by growing different sets under various conditions of illumination and by modifications of the chemical nutrients supplied.

Materials and Methods

There are two distinct phases in the establishment of the association between the oat plant and the oat smuts: (1) the phase of penetration of the germ tube of the fungus into the host; (2) the phase of the subsequent development of the mycelium within the host tissues. All the evidence points to penetration taking place in the young seedling stage, usually through the coleoptile while it is 2 cm. or less in length. If the mycelium succeeds in reaching the embryonic tissue of the oat plant, it continues to grow along with the latter, and thus a very intimate relation is established between the growing tissues of the two organisms which continues for weeks or even months. At the stage of flowering, with the development of the young ovule, the mycelium of the fungus grows very rapidly and soon passes into the spore-forming stage. The final result is the replacement of the flowering parts by the dusty mass of black spores. In the covered smut, the glumes are left somewhat intact, while in the loose smut they are more or less completely destroyed.

The investigations of Bartholomew and Jones (1923), Jones (1923), and Reed and Faris (1924) have demonstrated that the environal factors of temperature, soil moisture and soil reaction, are very important in the initial penetration of the oat smuts into the host plant. A temperature of approximately 20° C., a low soil moisture content, and a neutral reaction of the soil seem to be most favorable. Sand has proved

to be an excellent substratum for germinating the seedlings, having several advantages over soil.

Special strains of oats, as identified by their seed numbers and which have been grown for many years, were used—*Avena sativa* L.: Black Mesdag (70), Canadian (119), Danish Island (149), Early Champion (150), Gothland (152), Monarch (161), Scottish Chief (124); *A. byzantina* C. Koch: Fulghum (129 and 1000); *A. brevis* Roth (289); *A. fatua* L. (968). The seeds were carefully selected and, after the hulls were removed, were inoculated with dry spores and planted in sand in paraffin cups with a moisture content of about 20 per cent of the holding capacity. They were germinated at a constant temperature of 20° C. The sand was thoroughly washed and sifted, passing through a 20 mesh sieve, but not through a 40 mesh.

The seeds were planted at a depth of about one inch, usually emerging from the sand in four days. Generally, 20 seedlings were grown in each cup. Two or three days later, when the seedlings were about an inch high, they were transplanted to pots or jars, where the plants were grown to maturity. Several cups of seedlings of a variety were needed for each experiment and, on transplanting, the seedlings from each cup of a variety were thoroughly distributed throughout the pots. In the earlier experiments, the plants were grown to maturity in 12 inch azalea pots which were filled with ordinary loam soil to which sand had been added. While it was low in fertility, it contained sufficient nutrients for the full development of the control plants. However, responses to the addition of various fertilizers were marked. In 1935 a few preliminary experiments were carried out in which sand was used as a substratum for growing the plants to maturity and in 1936 and 1937 this material was used exclusively. When soil was employed as a substratum there were, of course, many unknown nutritive materials present. To a less extent, this was true also in the sand cultures, since traces of various substances might remain after the washing.

The existence of racial specialization in both species of smuts has been demonstrated by Reed (1924, 1929), Reed and Stanton (1936), Sampson (1925) and Nicolaisen (1931). Some oat varieties are highly susceptible to one specialized race and correspondingly resistant to another. Accordingly, the experiments were carried out with specific races of *Ustilago avenæ* (Pers.) Jens. and *U. levis* (K. & S.) Magn., specialized Race 1 of each of these species being used in most experiments. Avenæ 1 at different times was used to inoculate Canadian, Scottish Chief, Danish Island, and Gothland. With the exception of Scottish Chief, which usually has given a variable percentage of infected plants, all of these varieties are fully susceptible to this race of smut. Levis 1 was used to inoculate the susceptible varieties Canadian, Early Champion and Monarch. Danish Island, a variety which does not give 100 per cent infection with this race, was grown in a few experiments. *Avena brevis* was also used to some extent. An occasional plant of this oat is attacked; infection, however, practically never occurs on the main stalk, appearing only on the lateral branches, which remain conspicuously shorter than the normal.

In some experiments other specialized races were used. Avenæ 9, the Fulghum race, is characterized by its ability to infect the Fulghum oats, although it rarely produces 100 per cent infection. Avenæ 10 is a loose smut characterized by its ability to attack Red Rustproof oats and *Avena fatua* (968).

Levis 2 was used to inoculate Monarch and *Avena brevis*, the former being resistant while the latter, under favorable conditions, gives 100 per cent infection. Levis 7 is a Fulghum race of covered smut, characterized by its peculiar abiiity to attack not only Fulghum strains of oats, but also Black Mesdag. It is unusual, however, to obtain 100 per cent infection on these varieties.

Experimental Results

In the course of investigations, extending over a period of years, on the influence of environal factors on the infection of

TABLE 1

RESULTS FOR 1933 WITH *Ustilago avenæ*—RACE 1

	Scottish Chief					Gothland				
	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.
	Inf.	N.				Inf.	N.			
1. Illuminated	24	32	20	5(1) [1]	25	27	—	20	20	100
2. Non-illuminated	28	34	19	11	58	31	—	20	20	100
3. Nitrogen added	48	59	20	4	20	44	62	19	18	95
4. Lithium chloride added	42	46	20	5	25	33	—	15	15	100
5. Zinc sulphate added	33	37	20	4	20	30	38	20	18	90
6. Potassium added	35	41	20	8	40	27	36	20	19	95
7. Phosphorus added	26	32	20 [2]	6	30	30	—	20	20	100
8. Magnesium sulphate added	24	29	20	9	45	22	—	20	20	100

Abbreviations used in tables: Ht., height; Pl., plants; Inf., infected; N., normal.

Started January 10. Plants grown in 12 inch azalea pots, filled with equal parts of soil and sand.

One set illuminated with 200 watt lamps February 1–11 from 4:30 to 10 P.M., February 12–April 1 from 5:30 to 10 P.M.

In the nutrition series, 1 liter of each solution was supplied February 1, 15, March 1, 15, 31. The solutions employed were 1 per cent $NaNO_3$, 0.5 per cent KCl, KH_2PO_4, and $MgSO_4$, 0.1 per cent LiCl and $ZnSO_4$ for the first two applications and 0.05 per cent for the others. See Figs. 2A and 2B.

[1] Figure in parenthesis indicates partially smutted plants.

[2] On March 1 received an application of magnesium sulphate in error.

TABLE 2

RESULTS FOR 1933 WITH *Ustilago levis*—RACE 1

	Early Champion					Monarch			
	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches	No. Plants	No. Inf.	Per Cent Inf.
	Inf.	N.							
1. Illuminated	24	—	20	20(5)	100	27	20	20	100
2. Non-illuminated	42	51	20	19	95	40	20	20	100
3. Nitrogen added	44	54	19	16	84	51	20	20	100
4. Lithium chloride added	35	50	18	15	83	37	20	20	100
5. Zinc sulphate added	42	42	20	19	95	45	19	19	100
6. Potassium added	38	57	20	15	75	30	20	20	100
7. Phosphorus added	38	48	20	19	95	41	19	19	100
8. Magnesium sulphate added	29	—	19	19	100	27	19	19	100

See footnotes to table 1.

TABLE 3

RESULTS FOR 1935 WITH *Ustilago avenæ*—RACE 1

	Scottish Chief			Gothland		
	No. Plants	No. Inf.	Per Cent Inf.	No. Plants	No. Inf.	Per Cent Inf.
1. Illuminated	20	16	80	20	20	100
2. Illuminated—$NaNO_3$ added	20	13	65	20	20	100
3. Non-illuminated	18	11	61	20	20	100
4. Non-illuminated—$NaNO_3$ added	20	12	60	19	19	100
5. Full nutrient solution	20	15(1)	75	20	19	95
6. Potassium omitted	20	15	75	20	20	100
7. Phosphorus omitted	20	10	50	20	20	100
8. Full nutrient—LiCl added	20	14	70	20	20	100
9. Full nutrient—$ZnSO_4$ added	20	16	80	20	20	100
10. Lithium nitrate	20	17	85	20	20	100
11. Lithium chloride	20	16	80	20	20	100
12. Zinc nitrate	20	13	65	20	20	100

Started January 15–19. Plants grown in 12 inch azalea pots filled with equal parts of soil and sand. One set illuminated with 200 watt lamps February 8–28 from 5:30 to 10 P.M., March 1–28 from 6 to 10 P.M. A 1 per cent solution $NaNO_3$ applied to Exp. 2 and 4.

Full nutrient solution was 1 g. $Ca(NO_3)_2$, 0.25 g. each $MgSO_4$ and KH_2PO_4 per liter. In Exp. 6, NaH_2PO_4 was substituted for KH_2PO_4, and in Exp. 7, KCl for KH_2PO_4.

In Exp. 8 and 9, 0.25 g. LiCl and $ZnSO_4$ per liter were added, and in Exp. 10, 11, and 12, solutions containing 0.25 g. $LiNO_3$, LiCl, and $Zn(NO_3)_2$ per liter were applied.

On February 11 and 12, 500 cc. of the solutions were applied to each of the pots; second and third applications of 1 liter were made on February 28 and March 19.

See Figs. 3A and 3B.

TABLE 4

RESULTS FOR 1935 WITH *Ustilago avenæ*—RACES 9 AND 10

	Race 9						Race 10		
	Fulghum (129)			Fulghum (1000)			*Avena fatua*		
	No. Plants	No. Inf.	Per Cent Inf.	No. Plants	No. Inf.	Per Cent Inf.	No. Plants	No. Inf.	Per Cent Inf.
1. Illuminated	20	15	75	19	17	89	19	10(2)	53
2. Illuminated—$NaNO_3$ added	19	16(1)	84	19	16	84	20	15(2)	75
3. Non-illuminated	20	15	75	20	18(2)	90	20	16(4)	80
4. Non-illuminated—$NaNO_3$ added	20	16(5)	80	18	15(4)	83	18	15	83
5. Full nutrient solution	19	14(1)	74	20	17(4)	85	18	15(7)	83
6. Potassium omitted	20	16(3)	80	19	15	79	20	10(1)	50
7. Phosphorus omitted	20	16(1)	80	20	19(3)	95	20	12(1)	60
8. Full nutrient—LiCl added	20	19(1)	95	19	17(3)	89	20	11(2)	55
9. Full nutrient—$ZnSO_4$ added	20	16(3)	80	20	20(5)	100	20	10(2)	50
10. Lithium nitrate	20	19(5)	95	18	14(2)	78	20	14	70
11. Lithium chloride	20	17	85	19	16(3)	84	20	11	55
12. Zinc nitrate	20	17(2)	85	19	15(1)	79	20	10(2)	50

See footnotes to table 3.

oats by the smuts, on the racial specialization of the smuts, and the inheritance of smut resistance in oat hybrids, many experiments have been carried out which had a bearing on some phases of the present work. The main experiments, however, were made in 1933, 1935, 1936 and 1937. In 1933 and 1935 the inoculated plants were grown in a mixture of loam soil and sand, while in 1936 a clean grade of builders' sand and in 1937 white beach sand was used as the sub-

TABLE 5

RESULTS FOR 1935 WITH *Ustilago levis*—RACE 1

	Danish Island			Monarch				*Avena brevis*		
	No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches	No. Plants	No. Inf.	Per Cent Inf.	No. Plants	No. Inf.	Per Cent Inf.
1. Illuminated	20	7	35	36	19	19	100	12	0	0
2. Illuminated—$NaNO_3$ added	18	8(4)	44	36	20	20	100	19	8(8)	42
3. Non-illuminated	20	7	35	47	18	18	100	20	10(8)	50
4. Non-illuminated—$NaNO_3$ added	20	7	35	50	18	18(1)	100	18	2(2)	11
5. Full nutrient solution	20	6	30	46	20	20	100	18	3(3)	17
6. Potassium omitted	20	4	20	46	19	19	100	14	5(5)	36
7. Phosphorus omitted	20	3	15	50	20	20	100	19	6(5)	32
8. Full nutrient—LiCl added	20	6	30	50	20	20	100	17	6(6)	35
9. Full nutrient—$ZnSO_4$ added	19	5	26	46	18	16	89	17	3(3)	18
10. Lithium nitrate	20	6	30	46	20	20	100	18	4(3)	22
11. Lithium chloride	20	5	25	47	20	20	100	14	0	0
12. Zinc nitrate	19	7	37	47	20	20	100	19	6(6)	32

See footnotes to table 3.

TABLE 6

RESULTS FOR 1935 WITH *Ustilago levis*—RACE 7

	Black Mesdag			Fulghum (129)			Fulghum (1000)				
	No. Plants	No. Inf.	Per Cent Inf.	No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.
							Inf.	N.			
1. Illuminated	20	2(2)	10	20	4	20	30	35	20	8	40
2. Illuminated—$NaNO_3$ added	20	8(8)	40	20	11(6)	55	37	41	20	13(13)	65
3. Non-illuminated	20	5	25	20	8(4)	40	36	40	20	9(1)	45
4. Non-illuminated—$NaNO_3$ added	20	9(6)	45	20	9(9)	45	43	49	19	7(6)	37
5. Full nutrient solution	20	5(3)	25	20	9(5)	45	45	48	19	11(6)	58
6. Potassium omitted	18	7(7)	39	20	8(5)	40	46	49	20	9(6)	45
7. Phosphorus omitted	20	3	15	20	6(2)	30	37	41	20	7(4)	35
8. Full nutrient—LiCl added	19	6(2)	32	19	7(6)	37	44	48	16	8(4)	50
9. Full nutrient—$ZnSO_4$ added	20	4	20	20	9(4)	45	32	39	20	12(6)	60
10. Lithium nitrate	20	4(2)	20	21	7(5)	33	39	45	20	7(3)	35
11. Lithium chloride	20	2	10	19	6(3)	32	38	41	20	8(5)	40
12. Zinc nitrate	20	1	5	20	8(5)	40	39	43	20	10(3)	50

See footnotes to table 3.

TABLE 7

RESULTS FOR 1936 AND 1937 WITH *Ustilago avenæ*—RACE 1

	Year	Scottish Chief					Gothland				
		Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.
		Inf.	N.				Inf.	N.			
1. No nutrient.....	1936	8	12	20	14(1)	70	11	12	18	17	95
	1937	7	13	14	11	79	12	—	15	15	100
2. Full nutrient.....	1936	38	51	20	12(1)	60	39	—	20	20(1)	100
	1937	22	24	15	12	80	25	—	15	15	100
3. No nitrogen.....	1936	9	13	20	12	60	12	—	20	20	100
	1937	10	11	15	14	93	14	—	15	15	100
4. Excess nitrogen...	1937	31	39	15	12(1)	80	29	—	15	15	100
5. No potassium.....	1936	41	49	20	15(1)	75	43	—	20	20(1)	100
	1937	25	26	15	13	87	22	—	14	14	100
6. Excess potassium..	1936	41	53	20	14	70	43	—	20	20	100
	1937	26	32	12	11(1)	92	25	—	13	13	100
7. No phosphorus....	1936	11	16	20	14	70	15	22	19	18	95
	1937	15	23	15	13	87	22	—	15	15	100
8. Excess phosphorus	1936	38	47	20	15	75	45	—	18	18	100
	1937	24	34	15	12	80	24	—	13	13	100

In 1936, duplicate sets were started on January 9 and 23. Plants were grown in 3 gallon crocks filled with well washed builders' sand.

The full nutrient solution consisted of 1 g. $Ca(NO_3)_2$, 0.25 g. each $MgSO_4$ and KH_2PO_4 per liter. In Exp. 5, NaH_2PO_4 was substituted for KH_2PO_4, and in Exp. 7, KCl for KH_2PO_4. In Exp. 6 and 8, 0.75 g. of KCl and NaH_2PO_4 per liter were added.

One liter of each solution was applied on January 22 in the first series, and in the first and second series on February 6, 20, and March 17.

In 1937, the experiments were started January 7. Plants were grown in 3 gallon crocks filled with well washed beach sand. The same solutions were used as in the previous year. In Exp. 4, 0.75 g. $NaNO_3$ per liter was added.

One liter was applied to the cultures on January 21, February 1, 15, and March 1.

See Fig. 4.

stratum. Brief reports on the results obtained have been made (Reed 1936, 1937).

The data are recorded in tables 1 to 9, and Figs. 1 to 4 illustrate the characteristics of the plants grown in some of the experiments. In the tables, information is given regarding the varieties of oats and the races of smuts used, with the percentages of infection obtained. The notes supply miscellaneous details regarding the substratum in which the plants were grown, the composition of the nutrient solutions and their method of application, as well as information regarding the periods of illumination.

TABLE 8

RESULTS FOR 1936 AND 1937 WITH *Ustilago levis*—RACE 1

	Year	Danish Island					Monarch				
		Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.
		Inf.	N.				Inf.	N.			
1. No nutrient	1936	15	14	20	2	10	13	7	19	18	95
	1937	10	12	15	8	53	13	—	15	15	100
2. Full nutrient	1936	37	52	20	4	20	31	56	20	19(1)	95
	1937	23	30	12	3	25	31	—	15	15	100
3. No nitrogen	1936	17	15	20	1	5	13	—	20	20	100
	1937	13	16	15	7	47	11	—	14	14	100
4. Excess nitrogen	1937	32	35	15	10	67	37	—	15	15	100
5. No potassium	1936	35	50	20	3	15	42	—	20	20	100
	1937	22	32	15	6	40	31	—	15	15	100
6. Excess potassium	1936	38	51	19	2	11	51	—	20	20(2)	100
	1937	27	38	15	9	60	37	—	14	14	100
7. No phosphorus	1936	16	18	20	4	20	14	16	20	17	85
	1937	19	33	15	8	53	17	—	14	14	100
8. Excess phosphorus	1936	33	53	20	2	10	43	34	20	19	95
	1937	26	37	15	10	67	30	—	14	14	100

See footnotes to table 7.

In most of the tables, data regarding the growth of the plants are supplied. The length of the main stem and the extent of tillering were used as criteria in determining the influence of the varied conditions. Usually, in such experiments, the dry weight of the plants is determined. In these experiments, however, such data would be quite unreliable on account of the great differences in the dry weight of smutted and normal plants.

Experiments on Illumination

An experiment was carried out in 1929 in which Canadian was inoculated with Avenæ 1 and Levis 1. The seed was

TABLE 9

Results for 1936 and 1937 with *Ustilago levis*—Race 7

	Year	Black Mesdag					Fulghum (1000)				
		Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.	Ht. Pl., Inches		No. Plants	No. Inf.	Per Cent Inf.
		Inf.	N.				Inf.	N.			
1. No nutrient.....	1936	14	17	20	15	75	15	14	20	8(3)	40
	1937	13	10	13	11(1)	85	13	18	15	11	73
2. Full nutrient.....	1936	39	42	20	9(1)	45	32	42	20	9(8)	45
	1937	30	27	15	14(2)	93	29	28	14	12(4)	86
3. No nitrogen.....	1936	18	12	20	18	90	17	18	19	8(3)	42
	1937	15	15	15	14	93	15	15	14	8(1)	57
4. Excess nitrogen...	1937	34	—	14	14(1)	100	26	30	15	14(7)	93
5. No potassium.....	1936	42	43	20	10	50	—	46	20	3(3)	15
	1937	32	31	14	12	86	25	34	15	12(4)	80
6. Excess potassium..	1936	40	39	20	15(2)	75	34	37	20	9(6)	45
	1937	34	36	15	12	80	27	44	15	14(6)	93
7. No phosphorus....	1936	15	14	20	13	65	11	16	20	8(3)	40
	1937	22	23	15	13	87	19	25	15	11(2)	73
8. Excess phosphorus	1936	39	39	20	11	55	31	39	20	6(1)	30
	1937	33	22	15	13	87	26	32	15	14(3)	93

See footnotes to table 7.

planted January 10 and one set of seedlings was grown under the usual greenhouse conditions and another was artificially illuminated from February 10 to April 1. The characteristic differences in the plants grown under the two conditions are

FIG. 1. Canadian inoculated with *Ustilago avenæ* 1. 1, non-illuminated; 2, illuminated.

illustrated in Fig. 1, the photograph being taken May 10. At that time, all the plants in the illuminated series were fully headed, while no heading had occurred in the plants of the non-illuminated series.

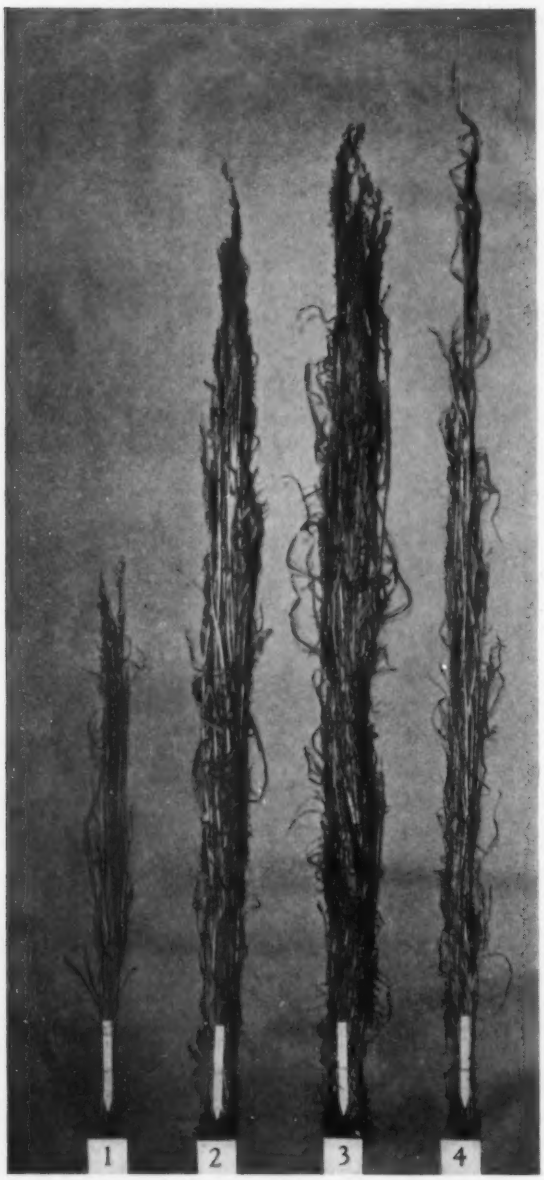


FIG. 2A. Influence of illumination and nutrition on Monarch inoculated with *Ustilago levis* 1. Plants grown in a mixture of soil and sand. (See Table 2.) 1, illuminated; 2, non-illuminated; 3, sodium nitrate added; 4, lithium chloride added.

FIG. 2B. Influence of illumination and nutrition on Monarch inoculated with *Ustilago levis* 1. (Continued.) Plants grown in a mixture of soil and sand. 5, zinc sulphate added; 6, potassium chloride added; 7, potassium phosphate added; 8, magnesium sulphate added.

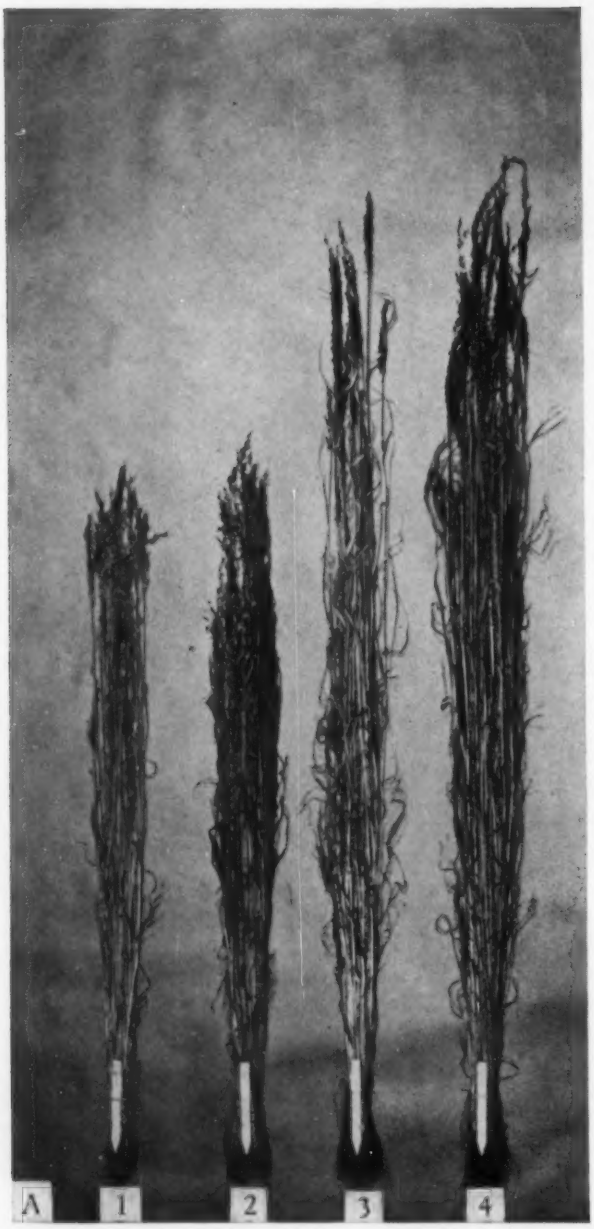


FIG. 3A. Influence of illumination and sodium nitrate on Monarch. Plants grown in a mixture of soil and sand. (See Table 5.) Inoculated with *Ustilago levis* 1. All plants infected. 1, illuminated; 2, illuminated—$NaNO_3$ added; 3, non-illuminated; 4, non-illuminated—$NaNO_3$ added.

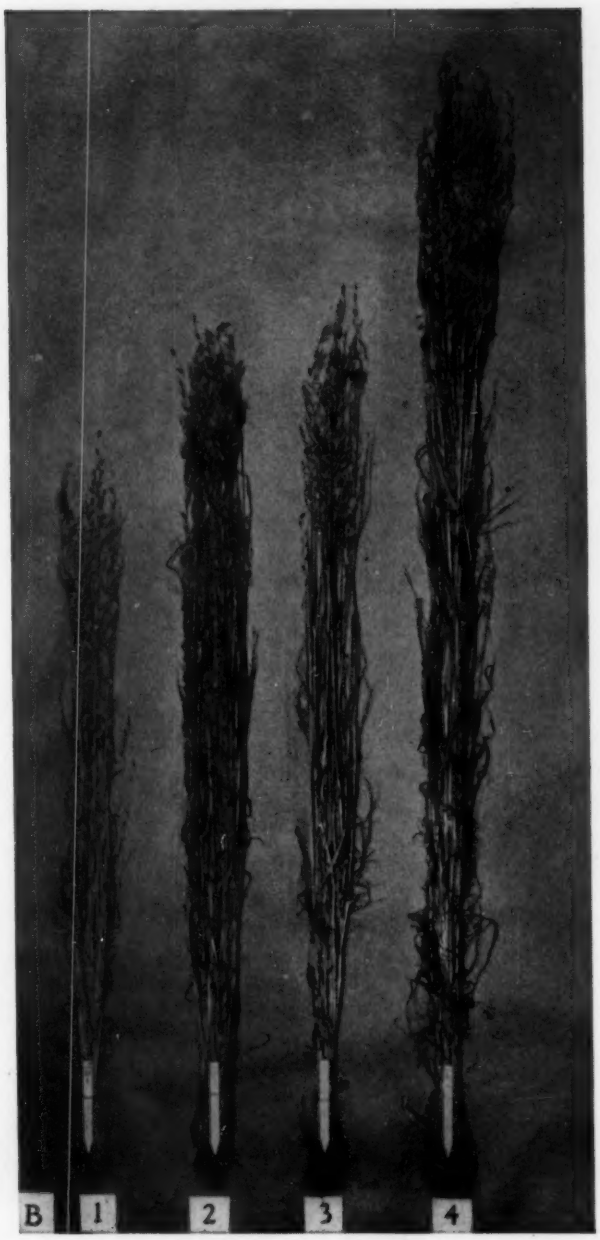


FIG. 3B. Influence of illumination and sodium nitrate on Monarch. (Continued.) Plants grown in a mixture of soil and sand. Inoculated with *Ustilago levis* 2. All plants normal. 1, illuminated; 2, illuminated—$NaNO_3$ added; 3, non-illuminated; 4, non-illuminated—$NaNO_3$ added.

FIG. 4. Influence of nutrition on infection of Monarch and Gothland. Plants grown in sand. (See Tables 7 and 8.) 1, *Ustilago levis* 1 on Monarch, full nutrient solution; 2, *Ustilago levis* 1 on Monarch, no nutrient solution; 3, *Ustilago avenæ* 1 on Gothland, full nutrient solution; 4, *Ustilago avenæ* 1 on Gothland, no nutrient solution.

Additional experiments were carried out in 1933, the data being recorded in Tables 1 and 2, and some of the plants are illustrated in Figs. 2A and 2B. A final series was grown in 1935, the data being recorded in Tables 3 to 6, and some of the plants illustrated by Figs. 3A and 3B.

The effect of illuminating the plants was to shorten greatly the time required for them to reach the heading stage. The difference varied with the varieties, but usually amounted to from two to four weeks. The illuminated plants produced relatively few tillers. In contrast, the non-illuminated plants, varying with the variety, usually produced one or more side branches, the main branches also being considerably taller. In 1935, a 1 per cent solution of sodium nitrate was added to one set of the plants in both series. The effect of the nitrogen was to increase the amount of growth and the extent of tillering.

These marked variations in the rate and extent of growth of the oat varieties showed no conspicuous difference in their effect upon the percentage of infected plants. In 1929, all the plants of Canadian in both series inoculated with Avenæ 1 and Levis 1 gave 100 per cent infection. Gothland, inoculated with Avenæ 1 in 1933 and 1935, gave 100 per cent infection in practically every experiment. Scottish Chief in 1933 gave somewhat higher percentages of infection in the non-illuminated as compared with the illuminated series, while in 1935 the reverse was true. The percentages of infection in the illuminated and non-illuminated plants supplied with sodium nitrate in 1935 were practically the same.

Usually, all of the plants of Monarch inoculated with Levis 1 were infected, and Early Champion also gave high percentages of infection. In 1935 no infected plants of *Avena brevis* were recorded in the illuminated series, while in the non-illuminated 50 per cent were smutted. However, in the corresponding series supplied with sodium nitrate 42 per cent infection of the illuminated plants was obtained and only 11 per cent of the non-illuminated. Danish Island in 1935 gave essentially the same result in all of the experiments.

In 1935, Avenæ 9 gave practically the same results on the two strains of Fulghum in the illuminated as compared with the non-illuminated plants, somewhat higher percentages being obtained in the series supplied with sodium nitrate. Avenæ 10 on *Avena fatua* gave somewhat higher percentages of infection in the non-illuminated series. Levis 7 seemed to give no consistent differences in the number of infected plants of Black Mesdag and the two strains of Fulghum in the illuminated and non-illuminated series.

Influence of Nutrition

Experiments on the influence of various nutrient salts, particularly those which contained nitrogen, potassium and phosphorus, were carried out. The extensive data obtained are recorded in tables 1 to 9. Essential details regarding the experiments are given in the footnotes. Figs. 2A to 4 illustrate some of the cultures grown in the different series.

The addition of nitrate to the cultures increased the height of the plants, bearing longer and broader leaves of a deeper green color. The plants tillered extensively, and the panicles produced were larger, with numerous spikelets. The difference between the plants supplied with nitrate was more marked in the sand cultures of 1936 and 1937 than in the soil cultures of the earlier years. Gothland, in 1936, growing in sand without any nutrient solution, averaged 11 inches in height; without nitrogen but with potassium and phosphorus supplied, 12 inches; while in the full nutrient solution the average height was 39 inches. An examination of the tables will reveal similar differences in the growth of the other oat varieties.

The results of growing the plants with and without phosphorus were also striking, particularly in the sand cultures. When Gothland was grown in a full nutrient solution, the plants averaged 39 inches in height, while in the experiment without phosphorus they averaged 15 inches.

There were no marked differences in the plants grown with and without a supply of potassium, especially in the soil

cultures. In the sand cultures of 1936, excess potassium appeared to make a difference in the total amount of growth.

The profound changes in the rate and extent of growth of the varieties in the various experiments did not seem to affect, in any significant way, the percentages of infection. Gothland inoculated with Avenæ 1 usually gave 100 per cent infection. Scottish Chief continued to give variable percentages of infection, but a detailed examination of the tables reveals no consistent relation to the supply of nitrogen, phosphorus or potassium. It may be noted, however, that the percentage of smutted plants was higher in 1937 than in previous years.

In 1935, two strains of Fulghum were inoculated with Avenæ 9, and *Avena fatua* with Avenæ 10. There were slight differences in the percentage of infection in the different experiments, but no indication that they were correlated in any way with the nutrients supplied.

Monarch and Early Champion, inoculated with Levis 1, usually gave 100 per cent infection. Danish Island, in 1935 and 1936, gave variable percentages of infection, but no relation to the nutrients supplied is indicated. In 1935 smutted plants of *Avena brevis* appeared in all of the experiments, the percentage varying from 17 to 36. In every case the infected plants were partially smutted, the main stem being normal.

Avena brevis, inoculated with Levis 2 in 1935 and 1937, gave complete infection of every plant in the experiments, while negative results were obtained on Monarch, except for one smutted plant in a single experiment which, however, may have been a stray infection with Levis 1. Black Mesdag and two strains of Fulghum were inoculated with Levis 7. There were some minor differences in the percentages of infection in the experiments, but no clear-cut relation to the nutrients supplied.

In some experiments, lithium chloride, lithium nitrate, zinc nitrate and zinc sulphate were applied to some of the cultures, the most extensive data being obtained in 1935 with all the different races of smut. The salts of lithium and zinc were likely to prove somewhat injurious to the plants and

consequently dilute solutions had to be used. The nitrates of both lithium and zinc produced a luxuriant growth of the varieties, indicating the effect of the nitrogen.

The data are recorded in Exps. 8 to 12 in Tables 3 to 6. In these experiments Gothland, inoculated with Avenæ 1, gave 100 per cent infection, while Scottish Chief varied from 65 to 85 per cent. Avenæ 9 on the two strains of Fulghum gave infections varying from 78 to 100 per cent; the percentage of infection of *Avena fatua* inoculated with Avenæ 10 varied from 50 to 70.

Monarch, inoculated with Levis 1, usually gave 100 per cent infection, while the percentage of smutted plants of Danish Island varied from 25 to 37, and of *Avena brevis* from 0 to 35 per cent. It may be noted, in connection with Table 5, that no stooling occurred in the two experiments where no infected plants were recorded. All the plants of *A. brevis* inoculated with Levis 2 were infected, while Monarch gave negative results. Levis 7, on the two strains of Fulghum, gave percentages varying from 32 to 60, and on Black Mesdag from 5 to 32.

Discussion

On the basis of the experimental data presented, the growth of the oat plant, greatly influenced by variations in illumination and modifications of the nutrients supplied, has no significant effect on the development of the oat smut within the tissues. Susceptible varieties such as Gothland, Monarch and *Avena brevis*, inoculated with specific races of the smuts, were fully infected whether the plants were 12 inches or less in height with no tillers and few spikelets in the panicles, or 4 to 5 feet tall with many side branches bearing broad leaves and large panicles with many spikelets. Although the growing period may have varied from a few weeks to several months, there was no evidence of any change in the susceptibility or resistance of these varieties to the races with which they were inoculated.

Scottish Chief inoculated with Avenæ 1, Danish Island and *Avena brevis* inoculated with Levis 1, Fulghum inoculated

with Avenæ 9, and *A. fatua* inoculated with Avenæ 10, in experiments extending over several years, have given a variable percentage of infected plants. Such varieties, perhaps, might be expected to show greater responses to specific changes in growth, resulting in a significant change in the number of smutted plants. Fulghum and *A. brevis* also tiller out very extensively, and thus may give a higher percentage of partially infected plants. There is no definite indication, however, that any of these varieties, inoculated with the specific races of smuts, have changed in their behavior. The percentage of infection still continued variable and without any relation to the particular treatment applied. In two experiments in 1935 (Table 5), *A. brevis* gave negative results with Levis 1. It is characteristic, however, of this species to be infected with this race of smut only in the lateral branches and, unless tillering takes place, no evidence of infection may appear.

Lasser (1937) has studied the influence of light and yarovization on the infection of certain cereals by the smuts. No conspicuous differences in the behavior of varieties of spring wheat, inoculated with *Tilletia tritici*, were recorded under any of the conditions, high percentages of infection usually being obtained in all of the experiments. On the other hand, four varieties of winter wheat, when subjected to yarovization, gave lower percentages of infection than the non-yarovized plants. However, no important variations in the percentage of infection under different conditions of illumination were recorded. Neither yarovization nor illumination had any obvious effect in influencing the amount of infection with loose smut of wheat and of barley. Lasser also recorded marked differences in the development of oat plants under various conditions of illumination and yarovization, but no significant variations in the percentages of smut were recorded.

The experiments of Gaines (1923) and Smith (1932) indicated that the long growing period of fall-planted wheat, as compared with spring-planted, may exert an influence upon the subsequent development of bunt. Faris (1924) also

offered some evidence for a similar condition in the covered smut of barley.

Many investigations have been carried out on the influence of fertilizers and mineral nutrients on rust and powdery mildew infection of various cereals. Spinks (1913) concluded that susceptibility to yellow rust on wheat and powdery mildew on wheat and barley was increased by supplying large amounts of available nitrogen, ammonium sulphate and sodium nitrate being equally effective. Potash salts appeared to decrease the susceptibility, although it did not counteract the effect of large quantities of nitrogeneous manures. Lithium salts seemed to produce immunity, while the nitrates of lead and zinc rendered the plants very susceptible. A resistant variety, however, tended to retain its immunity to yellow rust even when supplied with an excess of nitrogeneous materials. Many other investigators have found similar effects produced by nitrogeneous fertilizers. Gassner (1915), Stakman and Aamodt (1924) and Armstrong (1922), as well as others, suggest that the spread of rust on plants supplied with nitrogenous fertilizers is favored because they are more vigorous, mature later, and provide excellent conditions for its development. Turner (1922), Trelease and Trelease (1928) and Lowig (1933) found responses in the reaction of some of the cereals, grown in various nutrient cultures, to the powdery mildew. Hursh (1924) and Lowig (1933) called attention to the changes in tissue structure which might account for the difference in infection of plants supplied with nitrates, as compared with those supplied with potash.

In all these experiments with rusts and powdery mildews it may be that the influence of fertilizers and various chemical elements may be concerned as much with providing conditions favorable for penetration as for exerting an influence on the fungus after it has once entered.

In considering the influence of the growth of the host on the subsequent development of certain diseases, it is important to recognize the difference in the relations between the parasites and their host. The rusts and powdery mildews of

cereals produce a very local infection. A small area is attacked and relatively few cells are invaded by the mycelium at any given point. The extent of the attack on the host will depend on the number of viable spores which are distributed over the surface of the leaves. The cycle of development also is very brief. Under favorable conditions it is three to four days in the powdery mildew, while in the rusts it may be six to ten days. It also may be noted that in the rusts the mycelium develops in the parenchyma tissues of the interior of the leaf, while the powdery mildew is a surface parasite, sending haustoria into the epidermal cells. Thus, the relations of rusts and powdery mildews to their hosts are quite different from those of the oat smuts, which penetrate the seedling stage and, if successful, continue their development for long periods of time in close association with the embryonic tissue.

Our investigations emphasize the importance of environal factors, such as soil temperature and the water content of the substratum, during the period of germination of inoculated seed, for the infection of a susceptible oat variety. The subsequent development of the oat plant does not seem to influence the fundamental resistance or susceptibility to the smuts.

Accordingly, the conditions during the phase of penetration of parasites must be controlled properly in order to draw any final conclusions regarding the influence of later growing conditions on their development.

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A STUDY OF THE ABSOLUTE DIMENSIONS OF ECLIPSING VARIABLES

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ABSTRACT

For more than eighty per cent of eclipsing variables only the relative dimensions are known, as determined from photometric observations. Our ignorance of the masses, the result of a lack of spectroscopic observations, has been the chief handicap to a determination of absolute dimensions (radii, masses, and densities).

The author has developed a formula for deriving the masses in an eclipsing system when the relative dimensions of the system and one of the spectra are known. The formula is based on the laws of motion, the law of black body radiation, and the mass-luminosity relation (here considered empirically). The masses computed from the formula and observed directly are shown to be in agreement, with some exceptions which are discussed in detail.

The observed masses are used to derive a radiation temperature scale for eclipsing stars. Finally, fifteen binary systems, of which two are spectroscopic binaries, not established eclipsing systems, are evaluated in respect to their absolute dimensions.

The paper discusses some of the largest stars the radii of which have ever been derived and some of the largest masses known for eclipsing variables. Very low temperatures are involved for some of the fainter components.

1. THERE are only 35 eclipsing variables for which the masses are known, computed on the double basis of the orbital velocities of both components and the relative elements derived from the light curve. There are, however, more than 25 eclipsing variables for which the orbital radial velocity for only one of the components has been observed; and there are more than 200 such systems for which only the relative dimensions are known.

2. Since general recognition of Eddington's mass-luminosity relation, the eclipsing variables have often been used to compare the observed with the computed values. The comparisons have been made by the method proposed by Eddington himself: the absolute magnitude is derived, and,

after application of a temperature correction, the bolometric magnitude so computed is compared with the magnitude predicted from the mass. In this way it was found that the mass-luminosity relation is of the same character for both eclipsing variables and double stars; the latter, however, show a somewhat narrower range—not more than 10—in their masses, so that a direct comparison between the two groups of binaries can be made only for a limited number of systems.

3. Usually in a study of eclipsing variables one adopts a temperature scale based on general spectroscopic observations. These variables, however, give an independent derivation of the temperature scale if the parallax is known. The possibility of using this method was suggested by Russell, Dugan, and Stewart [1] in 1927 for β Aurigæ, and it has been applied independently by the author [2] for more than thirty components of eclipsing systems.

4. The three important problems, mentioned above under (1), (2), and (3), can be treated now in a new way.* A partial discussion of the method has previously been given by the author.[3] We write the following relations for a double star:

$$m_1 + m_2 = c_1 A^3 P^{-2} = m_1(1 + \alpha), \tag{1}$$

$$L = c_2 R^2 T_e^4, \tag{2}$$

$$L = c_3 m^{3+p}, \tag{3}$$

$$R = rA. \tag{4}$$

We replace A in (1) by R from (4); then, replacing R by L and T_e from (2), and, finally, replacing L by m from (3), we obtain the following general formula for computing the

* *See* partial investigations of these problems by Holmberg,[4] Pilowski,[5] Kreiken,[6] and Elsworth.[7]

[1] *Astronomy*, Russell, Dugan, and Stewart, II, 750, 1927.
[2] *A. N.*, **248**, 213, 1933.
[3] *P. A. A. S.*, **9**, 121, 1938.
[4] *Lund Medd.*, Series 2, No. 71, 1934.
[5] *A. N.*, **261**, 18, 1936.
[6] *A. N.*, **259**, 349, 1936.
[7] *Journ. d. Obs.*, **21**, No. 1, 1938.

mass (m) of a component in a binary system for a given period (P), temperature T_e, mass ratio (α), and radius of the star in the unit of distance between the centers (r):

$$\log m = 2/(7 + 3p)[-\log c_1 + 3/2 \log c_2 - 3/2 \log c_3 + \log (1 + \alpha) + 2 \log P + 6 \log T_e + 3 \log r]. \quad (5)$$

If the period is expressed in days, masses and distance between the centers (A) in solar units, then c_1 is 74.4^{-1}. If L, R, and T_e are expressed in solar units, then $c_2 = 1$. The third equation is the mass-luminosity relation, and if here again all quantities are expressed in solar units, $c_3 = 1$. If, however, every quantity in (1), (2), and (3) is expressed in grams, seconds, ergs, and degrees, then $c_1 = 5.93 \times 10^8$, $c_2 = 7.21 \times 10^{-4}$, and $c_3 = 4.83 \times 10^{-67}$. It is much more convenient to use the first system of units. Then we have:

$$\log m = 2/(7 + 3p)[1.871 + \log (1 + \alpha) + 2 \log P + 6 \log T + 3 \log r]. \quad (6)$$

Of the three equations (1), (2), and (3), the two former are exact, the third is only an approximation; for p is not exactly known, and is probably not constant throughout the whole interval of the masses used for eclipsing variables. Double star material gives $p = 0$ and $p = 0.2$, which are equally good first approximations. See, for example, Parenago.[8]

It is evident that the quantity α influences m in a very slight manner, for it is always less than 1. It will be shown below that α can be deduced, within the necessary limits of accuracy, from the data of the light curve. The quantities P and r are derived from the light curve, so that if the temperature can be adopted from the spectrum, we will be able to compute the masses without being dependent on spectroscopic orbital velocities. Thus Problem (1) can be extended over almost three hundred eclipsing variable stars.* Problem (2) can be treated differently: instead of comparing absolute

[8] *Rus. Ast. Journ.*, **14**, 33, 1937.

* The writer is now engaged in such computations.

magnitudes we can compare masses. Problem (3) can also be treated in a new way, as we have pointed out above.

TABLE I(*a*)

THE DATA OF 35 ECLIPSING SYSTEMS

Star	Period	Spectra	Amplitudes			Masses	
	d		*A*	*A*		m_1	m_2
S Ant	0.65	A8+A8	0.16	−0.09	*pl*	0.75⊙	0.42⊙
σ Aql	1.95	B8, B8+B8	0.44	0.40	*v*	6.19	5.14
TT Aur	1.33	B5, B3+B3	0.96	0.37	*p*	6.70	5.3
WW "	2.53	AO, A7+A7, A4sp	0.64	0.52	*v*	2.20	1.90
AR "	4.13	B9, AO+AO	0.74	0.54	*p*	2.68	2.50
β "	3.91	AOp+AOp, A1np	0.09	0.09	*pl*	2.40	2.36
ζ "	972.	KO+B1, cK4+B, K5+B9	0.70	—	*p*	16.4	9.5
i Boo	0.27	G2+G2	0.42	0.42	*p*	0.64	0.64
RS CVn	4.80	F8, F4n+dG8	1.92	0.08	*p*	1.85	1.74
TV Cas	1.81	B9, AO+AO, B5+B9	1.05	0.09	*v*	1.83	1.01
AO "	3.52	BO, O8, 5nk+O8, 5nk	0.2	0.2	*pl*	36.3	36.3
UW CMa	4.39	Oe, O7sfk+O7sfk, O9sfk	0.34	0.33	*p*	40.3	30.8
VV Cep	7430	Map, M2e+B9	0.8	—	*p*	44.5	35.6
AH "	1.77	B3, BOnk+BOnk	0.22	0.16	*v*	14.6	12.7
U CrB	3.45	B8, B5+B9	1.02	—	*v*	5.3	2.0
Y Cyg	3.00	B2, O9nnk+O9nnk	0.60	0.40	*v*	17.6	17.4
GO "	0.72	AO, B9n+AOn	0.54	0.23	*p*	1.04	0.88
MR "	1.68	AO, AO+AO	0.71	0.15	*p*	3.03	2.59
YY Gem	0.81	dM1e+dM1e	0.6	0.6	*p*	0.63	0.57
Z Her	3.99	F5p, F2p+F2p	0.80	0.12	*v*	1.6	1.3
RX "	1.78	AO, AO+AO	0.68	0.60	*v*	2.2	1.9
TX "	2.01	A5, A2+A2	0.70	0.34	*p*	2.06	1.77
u "	2.05	B3, B3+B3	0.68	0.28	*pl*	7.52	2.87
RT Lac	5.07	G5+K1	1.06	0.61	*v*	5.00	1.00
AR "	1.90	G5, G5+KO, G3+G2	1.03	0.29	*p*	1.42	1.41
CM "	1.60	A2+A2	0.85	0.20	*p*	2.00	1.50
U Oph	1.68	B8, B5nk+B5nk	0.69	0.59	*v*	5.36	4.71
WZ "	4.16	GO, GO+GO	0.74	0.72	*v*	1.41	1.35
AG Per	2.03	B3, B3k+B3k	(0.30)	(0.23)	*p*	5.2	4.6
V Pup	1.45	B1, B1+B3	0.64	0.53	*p*	21.2	16.3
U Sge	3.38	B9, B9+G2	2.88	0.07	*v*	6.70	2.03
W UMa	0.33	GO, F8p+F8p	0.76	0.68	*p*	0.75	0.54
TX "	3.06	B9+A3	2.49	—	*p*	3.0	1.0
Z Vul	2.45	B3, B3+B3	1.63	0.14	*v*	5.34	2.36
RS "	4.48	B8, B8+B9	0.73	0.08	*v*	5.26	1.64

The observational material is given in Tables I(*a*) and I(*b*) for thirty-five eclipsing stars. The period (Table I(*a*), column 2) is the essential element in Formula (6). The

spectra (column 3) are taken from classifications made at different observatories; the first determination usually refers to the Harvard classification. If both components are classi-

TABLE I(*b*)

THE DATA OF 35 ECLIPSING SYSTEMS

Star	α	$1+k$	*Ell.*	r^{-1}	L_1	No.	α_c
S Ant	0.56	1.73	0.78	2.80	0.67	1	0.73
σ Aql	0.81	2.00	0.95	4.45	0.57	1	0.89
TT Aur	0.79	1.86	0.83	3.88	0.73	3	0.63
WW "	0.86	1.96	1.00	5.62	0.58	2	0.87
AR "	0.93	2.09	1.00	9.56	0.51	1	0.98
β "	0.99	2.00	0.98	6.80	0.50	3	1.00)
ζ "	0.59	1.025	1.00	5.01	(0.52)	3	(0.97
i Boo	1.00	1.86	0.92	3.20	0.50	1	1.00
RS CVn	0.94	2.56	(0.93)	5.02	0.73	5	0.63
TV Cas	0.55	2.22	0.95	4.00	0.92	8	0.33
AO "	0.93	1.65	0.93	1.69	0.74	1	0.62
UW CMa	0.75	1.83	0.88	2.68	0.60	1	0.84
VV Cep	0.8	1.01	1.00	2.80	(0.50)	1	(1.00)
AH "	0.87	1.75	0.87	3.37	0.70	1	0.68
U CrB	0.38	3.08	0.97	6.70	0.90	6	0.36
Y Cyg	1.00	2.00	1.00	6.10	0.58	2	0.87
GO "	0.85	1.66	0.90	2.66	0.62	2	0.81
MR "	0.85	1.71	1.00	3.30	0.87	2	0.41
YY Gem	0.91	1.89	1.00	5.00	0.56	1	0.90
Z Her	0.81	1.70	1.00	4.88	0.70	4	0.68
RX "	0.86	2.00	0.95	5.23	0.52	4	0.97
TX "	0.88	2.01	1.00	7.18	0.55	4	0.92
u "	0.38	2.11	0.93	3.20	0.67	9	0.73
RT Lac	0.20	2.00	0.93	3.57	0.59	4	0.86
AR "	1.00	2.61	1.00	7.05	0.65	2	0.76
CM "	0.75	1.80	1.00	5.30	0.62	1	0.81
U Oph	0.89	2.11	0.93	4.59	0.60	2	0.84
WZ "	0.96	1.98	1.00	12.1	0.51	1	0.98
AG Per	0.90	1.70	1.00	3.78	0.60	1	0.84
V Pup	0.77	1.91	0.87	2.55	0.60	2	0.84
U Sge	0.30	2.33	1.00	4.57	0.92	4	0.33
W UMa	0.72	1.80	0.82	2.73	0.62	6	0.81
TX "	0.33	2.43	1.00	7.10	0.96	1	0.26
Z Vul	0.44	2.73	0.90	4.54	0.79	5	0.54
RS "	0.31	3.00	0.97	5.00	0.65	6	0.76

fied, they are connected with a plus sign. One sees from the column of spectra how flexible are some of the types, and it has been difficult to give preference to any one system of classification. From considerations developed below, how-

ever, it would seem that for most eclipsing binaries the Harvard system represents a more uniform temperature scale than other classifications. The amplitudes (column 4) are given in order to show how accurate are the relative elements. The symbols *pl*, *p*, *v* denote photoelectric, photographic, and visual observations.

The column headed $(1 + k)$ in Table I(b) serves the double purpose of showing the relative sizes of the two components, and whether the heavier component is also larger and brighter; if $(1 + k)$ is greater than 2, it means that the heavier component is smaller. The third column, *Ell.*, gives the mean value of the radius in an elliptical system according to the formula:

$$r = \sqrt[3]{abc}$$

in units of a. The succeeding columns give respectively the relative total brightness of the heavier component, the number of light curves used in obtaining mean values of L and k, and the computed mass ratio, derived from L.

The wide variety in the peculiarities of individual systems, as shown in Tables I(a) and I(b), does not permit of a detailed discussion of single stars at present. Such a discussion will be published later. It is possible to give here only a few general remarks.

The systems S Antliæ and β Aurigæ, though well observed photoelectrically, have such small amplitudes that there is always uncertainty in deducing their absolute dimensions. The ratio of the radii has been adopted after an investigation by the author of all light curves published elsewhere; the ratio for i Bootis was re-computed. The elements of AO Cassiopeiæ, derived by Pearce, are somewhat doubtful because of uncertainty in the light curve used by him. A new set of photoelectric observations by Bennet at Yale has confirmed the range of the light variation, so that there is little expectation that the absolute dimensions given by Pearce will be much changed; but the uncertainty due to the small range is not wholly removed. The system UW Canis

Majoris is uncertain for the same reason, though probably to a less degree than AO Cassiopeiæ, for the depths of minima in UW Canis Majoris are much greater than those in AO Cassiopeiæ. A new photoelectric light curve would contribute toward more precise values.

The mass ratio for VV Cephei, the heaviest of the known eclipsing systems, has not been determined with quite enough certainty, though all results seem to lead to the conclusion that it is not far from the real value. Probably a new photoelectric light curve should be obtained for AH Cephei. The dimensions of MR Cygni have been derived by the writer on the basis of the relative elements given by Jonson and Kopal. WZ Ophiuchi, the most recently discovered eclipsing system, has already been discussed by the writer.[9] V Puppis is not yet known spectroscopically with enough certainty. The elements for TX Ursæ Majoris have also been derived by the writer.*

It was said above that there is a correlation between α and L_1. Hypothetically the mass ratio may have any value, but statistically, as seen in Table I, for the majority of the systems the value is much greater than a half. Plotting the values of L and α and adopting a linear relation, we find the following formula for computing the mass ratio from the relative total brightness of the heavier component:

$$\alpha = 0.20 + 1.6(1 - L_1). \qquad (7)$$

A comparison between the computed α_c and the observed values of α given in Table I(b) leads to the conclusion that for 21 systems the computed α does not deviate from the observed by more than ten per cent. When we remember that Equation (6) is very insensitive to changes in α, it is permissible to conclude that α may be derived with considerable certainty from the light curve in computing the masses. We should

[9] H. B. 907, 1938.

* Much has been published in regard to the eclipsing variables. Some authors do not agree completely with others in their results, so that there is wide variation (as much as 20 per cent) in the derived absolute dimensions for certain systems. In order to eliminate this difficulty we have taken mean values, with proper consideration for the accuracy of the individual light curves.

note that some systems, such as u Herculis, RT Lacertæ, and RS Vulpeculæ, show a large positive difference between the computed and the observed α's, whereas RS Canum Venaticorum, AO Cassiopeiæ, and AR Lacertæ show a large negative difference. These systems are not entirely well established, but even such large differences do not influence the computed masses very much.

TABLE II

COMPUTED MASSES OF THE HEAVIER COMPONENTS FOR 35 ECLIPSING VARIABLES

Star	Adopted Temp.	Computed Mass		Observed Mass	Star	Adopted Temp.	Computed Mass		Observed Mass
		$p=0.0$	$p=0.2$				$p=0.0$	$p=0.2$	
		$\odot$	$\odot$	$\odot$			$\odot$	$\odot$	$\odot$
S Ant	7800°	1.98	1.88	0.75	YY Gem	3200°	0.31	0.37	0.63
σ Aql	12400	5.81	5.07	6.19	Z Her	7100	2.98	2.74	1.6
TT Aur	18000	9.57	8.05	6.70	RX Her	11000	3.90	3.52	2.2
WW Aur	8500	2.32	2.17	2.20	TX Her	9900	2.70	2.50	2.1
AR Aur	11000	3.93	3.53	2.68	u Her	18000	14.3	11.6	7.52
β Aur	10400	4.07	3.61	2.40	RT Lac	5200	2.70	2.50	5.00
ζ Aur	3300	17.6	14.0	16.4	AR Lac	5400	0.89	0.90	1.42
i Boo	5700	0.54	0.57	0.64	CM Lac	9100	2.95	2.72	2.07
RS CVn	6700	2.88	2.65	1.85	U Oph	15000	7.12	6.31	5.36
TV Cas	11000	4.38	3.91	1.83	WZ Oph	6000	1.12	1.09	1.41
AO Cas	26000	65.0	47.3	36.3	AG Per	18000	12.0	10.0	5.2
UW CMa	25000	51.4	37.9	40.3	V Pup	21400	22.4	17.6	21.2
VV Cep	2500	48.8	36.3	44.3	U Sge	11700	6.63	5.73	6.70
AH Cep	18000	13.0	10.6	14.6	W UMa	6100	0.88	0.90	0.75
U CrB	15000	6.97	6.00	5.3	TX UMa	11700	4.40	3.90	3.03
Y Cyg	24500	18.8	15.0	17.6	Z Vul	18000	11.3	9.38	5.24
GO Cyg	11700	4.65	4.13	1.04	RS Vul	12400	5.62	4.92	4.6
MR Cyg	11000	5.47	4.80	3.03					

The results for the masses of the heavier components are given in Table II. The computed mass, as may be seen from the table, is in better agreement with the observed value for the mass-luminosity relation when $p = 0.2$ than when $p = 0.0$, especially for large masses. There are three sources of error in the computed masses. First, the error may lie in the temperature scale used in the computation. The scale used here was that given by Russell, Dugan and Stewart.[1] There are other temperature scales which give higher values

for a given spectral class, so that a larger discrepancy between the observed and computed masses would result from their use. Secondly, the uncertainty may lie in the systematic error of classification—the spectra may be taken too early. Thirdly, the error may arise from the reflection effect. If we take into consideration all these uncertainties, we may be satisfied with the general accuracy of our results. We may note further that rather uncertain systems, such as ζ Aurigæ, UW Canis Majoris, and VV Cephei show a close agreement, but that such systems as β Aurigæ, TV Cassiopeiæ, u Herculis, and Z Vulpeculæ, for which the values are considered as the best determined, show poor agreement. Either they are not actually the best determined, or the systems are peculiar. GO Cygni is in a special category—the computed mass is more than four times greater than the observed. This is the largest disagreement to be found in Table II. According to the period, the spectrum should be much later; and yet there is no evidence of error in the observational results.

5. Section 4 was concerned with the problems mentioned in Sections 1 and 2. We now proceed to the problem of temperature alluded to in Section 3.

The temperature can be computed by the following formula:

$$6 \log T = (7 + 3p)/2 \log m - \log (1 + \alpha) - 2 \log P - 3 \log r - 1.871. \quad (8)$$

The designations are the same as before (T is in units of the solar temperature = 6000°). Table III gives our results for 70 components of 35 eclipsing systems, computed on the basis of the two values of p indicated. If any other $p = x$ be used, then the following relation would apply:

$$\log T_{p=x} = \log T_{p=0.0} + x/4 \log m.$$

The results in Table III are interesting in two ways. First, such stars as Y Cygni, i Bootis, and V Puppis, which may be regarded as among the best determined systems, show good agreement. Secondly, there is a tendency for the temperatures of most eclipsing variables to be lower than would be expected from the usual criteria.

TABLE III

TEMPERATURE OF 70 COMPONENTS OF 35 ECLIPSING SYSTEMS

Star	Logarithm of Temperature				Star	Logarithm of Temperature			
	Heavier		Lighter			Heavier		Lighter	
	$p=0.0$	$p=0.2$	$p=0.0$	$p=0.2$		$p=0.0$	$p=0.2$	$p=0.0$	$p=0.2$
S Ant	3.65	3.66	3.42	3.46	YY Gem	3.68	3.67	3.68	3.67
σ Aql	4.11	4.15	4.04	4.08	Z Her	3.70	3.70	3.66	3.67
TT Aur	4.16	4.21	4.04	4.08	RX "	3.86	3.89	3.86	3.87
WW "	3.90	3.91	3.86	3.87	TX "	3.92	3.94	3.83	3.84
AR "	3.95	3.97	3.91	3.93	u "	4.09	4.13	3.80	3.82
β "	3.88	3.91	3.83	3.85	RT Lac	3.87	3.90	3.44	3.44
ζ "	3.50	3.54	4.13	4.18	CM "	3.90	3.91	3.72	3.73
i Boo	3.73	3.71	3.74	3.73	AR "	3.86	3.87	3.65	3.66
RS CVu	3.72	3.73	3.56	3.57	U Oph	4.14	4.18	4.03	4.06
TV Cas	3.82	3.83	3.54	3.55	WZ "	3.83	3.85	3.83	3.85
AO "	4.27	4.34	4.33	4.41	AG Per	4.04	4.08	4.10	4.12
UW CMa	4.33	4.41	4.32	4.39	V Pup	4.35	4.41	4.16	4.19
VV Cep	3.34	3.44	4.24	4.32	U Sge	4.10	4.11	3.67	3.68
AH "	4.30	4.35	4.30	4.36	W UMa	3.75	3.75	3.64	3.63
U CrB	4.11	4.14	3.61	3.63	TX "	4.02	4.05	3.20	3.20
Y Cyg	4.38	4.44	4.38	4.44	Z Vul	4.04	4.08	3.82	3.83
GO "	3.64	3.69	3.72	3.73	RS Val	3.97	4.01	3.49	3.50
MR "	3.89	3.91	3.91	3.93					

Since the spread of temperature for a given spectral class, especially for stars later than Class A, is large, we can deduce two temperature limits in agreement with Table III. The results are given in Table IV. The low character of the temperatures is partly established through two supergiants, ζ Aurigæ and VV Cephei; the rest of the contributors are not

TABLE IV

THE DEDUCED TEMPERATURE SCALE FROM 70 ECLIPSING COMPONENTS

Spectrum	Temperature	Spectrum	Temperature	Spectrum	Temperature
O7	28000°	F5	6300° high	K0	4600° high
B0	21000		3300 low		3800 medium
B5	14700	G0	5750 high		2700 low
A0	10000		3000 low	K5	4000 high
A5	8400	G5	5100 high		3300 medium
A8	7900 high		4200 medium		2500 low
	6600 low		2900 low	Me	3400 high
F0	7400 high				3000 medium
	6200 low				2350 low

supergiants in respect to mass and luminosity, although they are much less dense than the sun. Even if the existence of such low temperatures is somewhat contrary to expectation, it is difficult to escape from the conclusion that they are probably in the neighborhood of eclipsing-variable temperatures. We may add that there are not many, if any, methods which give reliable results for temperatures of the type discussed here; and that deviation from black-body radiation for eclipsing variables may have a much more pronounced effect than has been found for δ Cephei variables.

6. There are several eclipsing systems which are unusual in certain respects, and the method of determining absolute dimensions outlined in the previous pages may be of some use in interpreting them.

TABLE V

Star	Period d	L_b	α_c	r_b	r_f	Sp_b	Sp_f	Remarks
ϵ Aur	9890	0.52	0.72	0.03	0.30	F5p	—	1
SX Cas	36.6	0.54	0.94	0.25	0.52	A6	G6	2
α CrB	17.4	1.00	0.20	0.05	0.02	AO	—	3
W Cru	198.5	0.90	0.36	0.61	0.34	GO	GO	4
GG Car	62.1	0.50	1.00	0.35	0.35	P Cyg	P Cyg	5
β Lyr	12.92	0.63	0.79	0.26	0.62	B8	B5	6
δ Ori	5.73	0.75	0.60	0.38	0.24	B1	—	7
β Per	2.87	0.92	0.34	0.21	0.24	B8	—	8
V 381 Sco	6475.	0.97	0.25	0.21	0.42	F5?	—	9
V 383 Sco	4900.	0.75	0.60	0.20	0.40	M?	—	10
RY Sct	11.12	0.60	0.84	0.34	0.34	Bq	Bq	11
λ Tau	3.95	0.85	0.44	0.27	0.27	B3	—	12
+ 6° 1309	14.4	0.55	0.92	0.30	0.30	O8	O8	13
+57° 28	55.9	0.70	0.68	0.20	0.27	B9	B5	14
−66° 3307	605.	0.85	0.44	0.20	0.27	P Cyg	P Cyg	15

Remarks

1. Elements taken partly from Güssow's paper (*Veröff. Ber. Bab.*, **11**, Pt. 3, 1936).

2. Elements taken from Dugan's work (Princeton Cont., No. 13, 1933).

3, 4, 6, 7, 8, 12. Elements taken from a compilation by the writer (*Veröff. Ber. Bab.*, **9**, Pt. 5, 1932) where all refer-

ences are given. In addition, see the paper by D. J. K. O'Connell (Publ. Riverview College Obs., No. 3, 1936) on W Crucis.

5. Elements adopted as arbitrary.

9, 10. Miss Swope's stars (H.B. 902, 1936); the elements of V 383 Scorpii are arbitrary.

11. See Harvard Tercentenary Papers, No. 27, 1937. The system has the spectrum of a nebula.

TABLE VI

THE ABSOLUTE DIMENSIONS OF FIFTEEN INTERESTING BINARIES

Star	Masses		Radii		Distance Between the Centers	Temperature of the Fainter Component
ϵ Aur	29.2⊙	21.0⊙	213⊙	2130⊙	7100⊙	1180°
SX Cas	13.7	12.7	35	75	138	5430
α CrB	3.24	0.7	2.3	0.6	46	3980
W Cru	34.1	12.2	313	158	513	3390
GG Car	58.2	58.2	111	111	317	14800
β Lyr	14.1	11.2	17	40	67	6300
δ Ori	36.2	16.7	19	12	47	16200
ζ Per	5.0	1.7	3.3	4.0	16	2250
V 381 Sco	104.	26.	1550.	3100.	7400	1460
V 383 Sco	20.4	12.2	760.	1520.	3800	1050
RY Sct	22.2	18.6	24	24	72	13000
λ Tau	14.6	6.6	8.0	8.0	29	9670
+ 6° 1309	59.0	54.3	36.	36.	120	24200
+57° 28	21.4	14.7	43.	57.	215	6920
−66° 3307	113.	49.7	326.	435.	1630	6400

13, 14. Plaskett's and Pearce's stars respectively. The relative elements are arbitrary, and may be regarded as maximum values.

15. Mrs. Mayall's star (Harvard Tercentenary Papers, No. 25, 1937). The relative elements are arbitrary, and may be regarded as maximum values.

We may point out that the relative elements given in Table V are good average values for eclipsing variables in general, so that one should not expect unusual results. From the relative elements given in Table V we obtain the data of Table VI, which contains the absolute dimensions of the systems considered here.

Concerning the masses, we see from Table VI that the most massive system studied is $-66°\,3307$; and that the next massive is V_{381} Scorpii. It is somewhat surprising that the most massive binaries hitherto found, $+6°\,1309$ and $+57°\,28$, here appear to be of relatively low mass, and that the computed masses for these systems are much less than those observed spectroscopically, approximately in the ratio one to four. If we should try to transform the ratio into one to one, attributing the difference wholly to the difference in temperature, then we would have to assume that the effective temperature for Class B9 is over 30,000°. We may add further that for these systems we have assumed the most favorable (maximum) values of the relative elements, and that the observed values are minima, so that if more probable values for the relative elements were adopted the above mentioned large difference would be even greater. The system $-66°\,3307$ is a P Cygni star and hence the usual conception of a continuous decrease in density and effective temperature seems to have little application here; that is, a P Cygni star (others perhaps also) has a "shell-like" structure. The same remarks are fully applicable to GG Carinæ. The systems α Coronæ Borealis, β Persei, and λ Tauri are not peculiar in absolute dimensions; indeed the values obtained here are in close agreement with those obtained originally by Stebbins, Eddington, and Slavenas. The system SX Cassiopeiæ is unusual in that, as a small value of the mass function was found spectroscopically ($f_2 = 0.48$), one would expect a rather low mass. We may here call to mind that this system shows bright lines of hydrogen in its spectrum.

Concerning sizes, V_{381} Scorpii seems to surpass all eclipsing variables in voluminal dimensions, though such systems as VV Cephei (radius = $2400\odot$; see H.C. 421), ϵ Aurigæ, and V_{383} Scorpii nearly approach it in size.

Finally, concerning the temperature, we see that the lighter component of the system V_{383} Scorpii seems to have the lowest temperature (1050), though of the same order as the temperature of the lighter component in ϵ Aurigæ and in

V_{381} Scorpii. Especially interesting is the system β Lyræ. Spectroscopically it is one of the heaviest systems, the mass function being over eight solar units. Spectroscopically also there is a second spectrum, originally called Class B5, which does not show an oscillation reflecting that of the B8 spectrum. Besides, there are bright bands, with absorption wings flanking them. All three types of spectra display very little correlation in their behavior, at least in an evident manner, so that several conclusions (Miss Maury, Curtiss, Baxandall, Struve) have been drawn about the existence of a large envelope around the system. The low temperature obtained here suggests that the second component must be of a much later spectral class than B5, probably F. From this result one is led to conclude that the visible spectrum of B5 character does not come from the second component. We should point out here that the computed mass of this system is much lower than would follow from the mass function. The spectrum of the first component must therefore be slightly earlier than B8.

Summary

A method has been outlined which allows for determination of the absolute dimensions of an eclipsing system, if the spectrum, period, and relative dimensions, derived from the light curve, are known (Formula (6)). Thirty-five eclipsing systems with known absolute dimensions have been used to obtain a temperature scale (Table IV). Fifteen interesting eclipsing variables, for which no determinations of absolute dimensions have previously existed, have been investigated (Section 6). It has been found that there are four systems in which the fainter component has a radius of over 1000 solar radii (V_{381} Scorpii, 3100; VV Cephei, 2400; ϵ Aurigæ, 2130; and V_{383} Scorpii, 1520), and that there are several faint components with temperatures less than 1500° absolute. β Lyræ, Plaskett's, Pearce's, and Mrs. Mayall's stars have been taken into consideration.

I am grateful for the opportunity of discussing the above results with Dr. M. Schwarzschild.

WITH AN ASTRONOMER ON AN ECLIPSE EXPEDITION *

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ABSTRACT

From eclipse expeditions in comparatively recent years there have been two outstanding achievements. The first was the verification of the Einstein general theory of relativity by the measurement of the deflection of the light from stars by the strong gravitational attraction of the sun. The Einstein theory not only has had important consequences for the physical sciences but also started a new trend of philosophical thought. The second result from eclipse expeditions has been the measurement of the heights in kilometers to which gases extend in the sun's atmosphere, the chromosphere. Coupled with great advances in knowledge of the structure of the atom, these heights gave to Saha the confirmation of his theory of ionization. The past two decades have, in consequence, been the most exciting and the most prolific years in the whole history of astronomy.

All the prominent lines in the solar spectrum have been assigned to multiplet groups with known excitation potentials measured in electron-volts from which have been ascertained the relative numbers of atoms engaged in the formation of the spectrum lines. Coupled with heights derived from eclipse spectra, the Evershed displacement in sun-spots has shown that there is a circulation of gaseous material near the sun's surface proving that each sun-spot is a gigantic vortex. The red shift from the Einstein theory is confirmed in the sun but the heights from eclipse spectra show that the strong lines, those of low excitation potentials extending to the greatest heights, in addition to the relativity shift show a motion of descent towards the sun while the weak lines of high excitation potentials which extend only to small heights have a motion of ascent. Within a strong multiplet group, there are important correlations with the result that the heights found directly from the flash spectrum or from the Evershed effect are not constant but are the greatest for the lines of greatest intensity which involve the largest number of atoms. A knowledge of heights also gives knowledge of the density distribution of various elements in the lower chromosphere, together with an indication that turbulence and not selective radiation pressure is responsible for keeping the different elements so well mixed.

SOLAR eclipse observers must always be confirmed optimists and good philosophers, or in other words, they must be willing to gamble at long odds on eclipse day with the fickle weather. Some astronomers have had a long run of bad luck. For instance, in 1936 in Japan, the sky at the time of the eclipse had been perfectly clear right up to the time of totality and then it suddenly clouded up. Similar conditions had

* The Penrose Memorial Lecture delivered April 22, 1938.

been met previously by three members of the British expedition with the result that in a total of twelve attempts to observe an eclipse there had been only one success. On the other hand, some members of the American Philosophical Society, notably W. W. Campbell and John A. Miller, at some eclipses have had remarkably close shaves from clouds, but miracles seem to have happened and when all hope was gone the skies suddenly cleared off for just a few moments at totality and then clouded up again for the rest of the day. In the observation of eclipses, I too have had a generous share of good luck.

In the article entitled "Nature's Most Dramatic Spectacle," [1] is given a popular account of the National Geographic Society-U. S. Navy expedition to Canton Island in mid-Pacific Ocean to observe the total eclipse of June 8, 1937. U.S.S. "Avocet" carried the thirteen members of the expedition and the equipment consisting of 150 cases of instruments, ten thousand board feet of lumber, sixty bags of cement, tents, cots and supplies for a six weeks' trip away from the base at Honolulu.

The scientific program carried out by the expedition consisted of the following:

Captain J. F. Hellweg, U. S. N. (retired), superintendent of the U. S. Naval Observatory, gave the signals for the four contacts and counted time.

S. A. Mitchell, University of Virginia, had three concave grating spectrographs, each used without slit, for photographing the spectrum of the chromosphere and the corona.

F. K. Richtmyer, Cornell University, had four identical lenses for photographing the corona, with three of which he had screens of "polaroid" rotated at different angles. The purpose was to measure the polarization in the corona. With illumination meters and a photoelectric cell, he measured the intensity of illumination during the progress of the eclipse from first to fourth contact and also

[1] *National Geographic Magazine*, *72*, 361, 1937.

the total light of the corona compared to that of the full moon.

Theodore Dunham, Jr., Mount Wilson Observatory and Charles G. Thompson, president of the Foundation for Astrophysical Research, New York, used a powerful spectrograph which was virtually a duplicate of that on the 100-inch Mt. Wilson reflecting telescope.

Irvine C. Gardner, National Bureau of Standards, Washington, D. C., photographed the corona in black and white and in colors by using a camera constructed under his direction of 9 inches aperture and 19 feet focal length.

Paul A. McNally, S.J., Georgetown University Observatory, photographed the corona through different color-filters and with plates specially sensitive to different spectral regions.

John E. Willis, U. S. Naval Observatory, photographed the partial phases of the eclipse and also the corona with a 6-inch aperture and 15-foot focus camera.

Charles Bittinger, Washington, painted the corona on a canvas 60 x 60 inches. The painting was on exhibit during the reception following the Penrose Memorial lecture.

R. H. Stewart, National Geographic Society, assisted Dr. Gardner during the total eclipse and also obtained a moving picture film of the eclipsed sun from first through to fourth contact.

In addition, the National Broadcasting Company had an announcer (George Hicks) and two engineers, Brown and Adams.

This was my fourth cruise on eclipse expeditions with the United States Navy, the three previous ones having been to Sumatra in 1901, Spain in 1905 and Niuafoou Island in 1930. Twice before I had been across the equator and had paid proper homage to Father Neptune so that in 1937 I was no longer a land lubber.

This 1937 eclipse will be memorable for four different reasons: first, for the longest duration of totality of any

eclipse in more than twelve hundred years (since the year 699); second, that it began on June 9 and ended on June 8, the day before it started; third, in spite of being in the tropics where clouds are the rule rather than the exception, the skies were clear at each and every station where there was an expedition; and fourth, in spite of isolation and long distances, interested persons could listen in on frequent broadcasts that told of the life on the island and of the progress of preparations over a coast-to-coast network. Two of the broadcasts were specially dramatic, one of fifteen minutes duration with totality at the middle of it, which gave a graphic description of the spectacular phenomenon, the other a few hours later when I, as the scientific leader of the expedition from Canton Island, held a broadcasted conversation with London half way round the world. In the great metropolis was a friend of many years standing, Alfred Fowler, a distinguished astronomer who in 1893 had made the first photograph of the flash spectrum, which since the year 1900 has been my own chief interest at eclipses.

On May 26, the party from New Zealand arrived aboard H.M.S. "Wellington." There were eight in the expedition, two of whom had worked alongside me on Niuafoou or "Tin-Can" Island at the eclipse of October 21, 1930. It was pleasant to renew old friendships.

Ashore we kept Honolulu time and we had an hour of daylight saving. As we were near the equator (3° South), the sun actually rose near six o'clock, but as our clocks said seven o'clock, everybody got up without grumbling and went to work immediately after breakfast.

On eclipse day we were awakened by a loud-lunged sailor at 4:40 A.M., though some members of the expedition had not been able to go to bed at all on account of last minute preparations. The skies had been clear the night before until after midnight. It was hopelessly cloudy when we emerged from our tents about five o'clock. There was a broadcast at 5:30 A.M., at which time we gave impressions of the weather at earlier eclipses observed and gave expression to our hopes

and expectations for clear skies. A hurried breakfast was at six o'clock under the light of lanterns, with the clouds somewhat broken but still bad towards the point of sunrise. At sunrise, conditions were greatly improved. We did not have long to wait to find what our luck was going to be for the reason that a quarter of an hour later the whole sky was clear except for floating clouds here and there. We were in rare good luck for the only clouds over the sun from first contact at 7:35, through to fourth contact more that two hours later, were a few thin ones, the first of which passed over the sun about three minutes after the precious photographs had all been taken during totality.

In the present article, attention will be confined to scientific discoveries made from eclipse expeditions in recent years and an attempt will be made to formulate the outstanding problems that await solution at eclipses in the years to come. In the James Arthur lecture delivered at the Smithsonian Institution in February, 1937 and published in the Smithsonian Annual Report, greater details are given for various phases of the work.

To the naked eye, the 1937 corona was circular in outline to an angular distance of one solar diameter. In addition there were many long spikes going out at various angles, the longest being two diameters in length. Looking through a hollow tube to protect the eye from sky illumination, a New Zealand observer on Canton Island saw one streamer extending to four solar diameters while a photograph taken by F. K. Richtmyer showed a spike extending to six diameters. From Peru, at an elevation of 25,000 feet, but on account of the low altitude of the eclipsed sun, with an air-mass three times that of a zenith sun at sea-level, Major Stevens obtained airplane photographs showing a "globular corona" extending to one solar diameter. At greater angular distances the long pointed streamers made a gorgeous spectacle, perhaps the most beautiful of my ten total eclipses. Although all the developed photographs showed many prominences, there were no huge masses such as occurred at the eclipses of 1918

and 1919, with consequent disappointment to those observers who were photographing the corona by color processes. On Canton Island, Gardner used three different methods. The developed photographs showed a greener corona than was visible to the eye. Gardner and also Michie of New Zealand used a rotating sector to cut down the brightness of the inner corona, a method which was tried first in 1889. The 1937 results were none too satisfactory.

The corona may be photographed by cameras large and small, fast or slow. With cameras of great length it is necessary to counteract the diurnal motion. This may be done by mounting the telescope horizontally and then using a plane coelostat mirror or by pointing the object glass directly to the sun. For moderate focal lengths up to 15 feet, a polar axis may be employed but for greater lengths a tower telescope is erected. The Swarthmore astronomers have obtained exquisite definition with focal lengths of 65 feet.

It is interesting to note that at eclipse expeditions success has always been greater in the direct photography of the corona than in any other branch of the investigation. The reason is evident, namely, that in every well-equipped expedition, no matter what the other program may be, the corona is always photographed. However, there is another reason not so apparent, which is that perfection of focus and conditions of "seeing," although desirable, are not absolutely essential in obtaining successful photographs of coronal details. A lack of perfect focus plays havoc with spectroscopic photographs but detracts little from the beauty of the coronal images for the reason that its structure is always nebulous or filmy in detail.

The general shape of the corona can be predicted [2] in advance of the eclipse depending on the sun-spot cycle. Recently it has been found that the "minimum type" of corona, with long equatorial streamers and stong polar brushes does not occur at the time of minimum of spots but 1.25 years earlier. Likewise, the corona closest in shape to a

[2] *Handbuch der Astrophysik*, 7, 396, 1936.

circle takes place 1.25 years before spot maximum. The eclipse of 1932 took place 1.2 years before minimum of spots and its shape was the pronounced elongated type. The succeeding total eclipse of 1934 was only 0.2 years after spot minimum and yet the corona had lost its minimum-type features and much more closely resembled the circular or "maximum-type" corona. Although the time of next sunspot maximum is still uncertain, it is highly probable that the eclipse of 1937 occurred within a year in advance of maximum. Its shape, almost exactly circular was the one confidently predicted.

Many attempts have been made to detect motions of coronal materials but without pronounced success. A comparison of Swarthmore and Lick photographs of the 1918 eclipse seemed to indicate motions of the order of 10 miles per second, but at the 1926 eclipse a comparison of coronal photographs [3] made in East Africa and Sumatra revealed that any motions in the corona were no more than one mile per second, or no greater that the inherent errors of measurement. The few spectroscopic measures that have been made seem to indicate motions away from the sun at speeds of 15 miles per second, though the evidence is not very convincing and needs to be verified.

Valuable information [4] regarding details of coronal radiation were secured in 1930 from large scale spectra taken with a concave grating without slit when the coronal lines are rings of light and not the customary straight lines one expects in spectra. The spectrograms were compared with direct photographs of exquisite detail in the corona and also with spectroheliograms taken on four successive days thousands of miles away at Mt. Wilson or Kodaikanal Observatories. All of the photographs tell of an intimate connection between coronal streamers and prominences but also that the streamers, on which the shape of the corona more or less depends, are always located near prominences but are not necessarily

[3] Ibid., 399.
[4] *Astrophysical Journal*, *75*, 1, 1932.

exactly connected with those prominences which at the time of the eclipse are of the greatest height. The eclipse spectra showed that the details of the coronal structure in the green "coronium" line at 5303 A differed greatly from the details found in the red line at 6374 A.

In the report of the Commission on Eclipses presented to the International Astronomical Union at the Paris meeting in 1935, it seemed that we might be on the verge of a fairly complete understanding of the method whereby the light of the corona is diffused and scattered. Grotrian had found that the distribution of energy in the spectrum of the corona is identical with that in the solar spectrum, the dark lines in the spectrum of the outer corona giving fair indication that the light of the corona is scattered sunlight. At the 1932 eclipse, Dufay and Grouiller had shown that the amount of polarization in the corona is entirely independent of wave-length and is a maximum of 26 per cent at an angular distance 10′ from the sun's limb.

Other observers, however, have come to quite different conclusions. From the two eclipses of 1932 and 1934, Cohn[5] finds that the polarization in the corona is quite different for short and for long wave-lengths. In fact, the polarization in the integrated light, in the blue and in the red increases steadily from the sun's limb toward the outer corona, whereas it reaches maxima both in the violet and in the green and then decreases again. At 4.5′ from the sun's limb the polarization is actually independent of wave-length but at smaller distances in the inner corona the polarization is higher for short wave-lengths than for long wave-lengths. At the 1936 eclipse Zakharin confirms the conclusions of Cohn that the degree of polarization is different for different regions of the spectrum.

Likewise our knowledge regarding the law of distribution of light within the corona is in a very unsatisfactory state. There are many laws of intensity found by various observers, such as the 2nd, 3rd, 6th, 7th, 8th and such powers as 2.3 or 2.4 of the distance measured either from the center or from

[5] Ibid., *87*, 284, 1938.

the edge of the sun. At the 1936 eclipse, the Polish expedition found that the intensity of the coronal radiation measured from the sun's limb outward diminishes more rapidly in red light than in yellow while the Italian photographs at the same eclipse showed that the intensity in the blue also diminished more rapidly than in the yellow. On the contrary at the same eclipse, the Japanese expedition [6] finds that the coronal light diminishes in intensity inversely as the 3.5 power in blue light and as the 5.5 power in yellow light, the distances outward being measured from the sun's center.

In view of much conflicting evidence it is very important that measures both of intensity and of polarization be repeated in different wave-lengths by many different observers. For polarization measures there has come a great simplicity of technique through the application of "polaroid" to eclipse work, which has been done both at the 1936 and 1937 eclipses.

Accurate values of the total light of the corona have been derived by Richtmyer and by Stebbins [7] in Peru, each using a photoelectric cell. The two results are in good agreement, namely, 53 and 47 per cent, respectively of the total light of full moon. At the eclipses of 1918 and 1925, the photoelectric cell used by Kunz and Stebbins gave values of 50 and 44 per cent, respectively. From the ten most reliable measures previous to 1937, the average showed the total coronal light 47 per cent of the full moon.

Measures by illumination meters of the intensity of light of the corona plus illuminated sky have given much conflicting information for the reason that it has been impossible to allow adequately for the effects of sky illumination. In spite of the conflicting evidence, however, some astronomers have reached too hasty conclusions in their attempts to correlate intensity of coronal radiation with sun-spot activity. It does indeed seem probable that the inner corona at spot maximum must be brighter than at spot minimum. Moreover, as the inner corona contributes the greatest part of the energy of the

[6] *Ann. Tokyo Obs.*, 2nd Ser., *1*, 91 and 139, 1938.
[7] *Astrophysical Journal*, *87*, 225, 1938.

total coronal radiation, we would logically expect that the total energy at maximum of spots would be greater that at minimum. Unfortunately, eclipses come but seldom and there are far too few observations from which to derive any satisfactory correlations.

In spite of many inconsistencies in the results, observers throughout the past fifty years have been in substantial agreement that the total light of the corona is roughly one-half that of the full moon, or one-millionth that of noon-day sun.

In the year 1882 a bright comet was observed near the eclipsed sun but was never seen before nor since. The corona appeared so bright that attempts were made to photograph it without waiting for an eclipse. Attempts followed attempts with every conceivable variety of attack—and failure to achieve results followed failure for nearly fifty years. In fact, it was not until 1930 that Lyot of the Meudon Observatory achieved a brilliant success where others had repeated failures. His great triumph resulted from the ingenious arrangement of his telescope to reduce to a minimum the amount of scattered light inside the instrument. A mountain observatory on the Pic du Midi (9100 feet) and skies at times very transparent permitted photographs of the brighter parts of the corona in full daylight but he also derived wave lengths of the emission lines of "coronium." Following his earlier successes, Lyot [8] has determined from the mountain observatory during the past three summers the wave-lengths of eleven lines with the following values and probable errors:

3388.10 ± .07	7059.62 ± .05
5116.03 ± .02	7891.94 ± .07
5302.86 ± .02	8024.21 ± .10
5694.42 ± .07	10746.80 ± .15
6374.51 ± .03	10797.95 ± .15
6701.83 ± .03	

The three last lines have not yet been photographed at eclipses. In the summer of 1937 Lyot obtained 125 spectra.

[8] *C. R.*, *202*, 392 and 1259; *203*, 1327, 1936 and *C. R.* 1938 (in advance of publication).

The line 5694 A was discovered this past year. It is faint but it appears with a dispersion of 7.5 A per mm. It is fainter than 5116 A which in turn was not seen in 1935 but was photographed both in 1936 and 1937. Lyot has not yet photographed the coronal line 5536 A which has been observed several times in the past thirty years with an intensity about equal to 5116 A. Lyot reports that the intensities of the coronal lines vary, the line 6702 A was stronger in 1937 than in 1936 and it was more intense than 7059 A. The line in the infra-red at 8024 A is generally quite feeble but it appears on several spectra taken under good conditions.

Lyot was the first to find that the green and red coronium lines 5303 and 6374 are broad lines. Later measures give the equivalent widths of 5303, 6374 and 6702 to be 0.80,0.97 and 1.07 A, respectively, the quotient of the equivalent width by the wave-length being very nearly a constant. He has also succeeded in the remarkable feat of photographing the 5303 line near sea-level at the Meudon Observatory.

The coronal line 7892 A was discovered by Curtis and Burns in 1925. It was photographed at the 1936 eclipse by Williams and in 1937 by Mitchell using the same spectrograph that was employed in its discovery in 1925. It is a strong line, in fact, it has a greater absolute intensity than 6374 A.

In addition to the eleven lines measured by Lyot, there are twelve other lines in the spectrum [9] of the corona that have been observed at many eclipses. Consequently, there are 23 known emission lines. In addition, at the 1936 eclipse two Japanese expeditions publish a total of 11 lines in the region between 5000 and 7000 A where Lyot's spectra were photographed under the best conditions. These lines are faint and need to be verified by future observations.

It may be said that the corona exhibits three separate spectra: first, the emission spectrum in the inner corona extending to 5′ or 200,000 km. from the sun's edge; second, the continuous spectrum (without lines) in the middle corona; and

[9] S. A. Mitchell, *Eclipses of the Sun*, p. 376, 1935.

third, the Fraunhofer lines showing feebly in the outer corona. In the continuous spectrum it has been found [10] that there is a band of absorption at wave-length about 6300 A which has a greater intensity in the corona than in the photosphere. At the same eclipse, this band was photographed [11] in the spectrum of the light of sky during totality. The same band appears in the spectrum of twilight, in the spectrum of the night sky and also in the spectrum [12] of the aurora borealis. In accordance with the theory of Störmer that aurorae are caused by corpuscular radiation from the sun, then it must happen that at the time of intense solar activity there would be a strong aurora on the side of the earth turned towards the sun. The band seen in the 1936 spectra needs to be confirmed at future eclipses but it does look probable that it is actually the auroral band which one would expect to be strong near the time of sun-spot maximum.

By comparing wave-lengths east and west, Lyot [8] finds, that the inner corona rotates at a speed about equal to that of the photosphere.

In comparing the green and red coronal rings obtained [4] in 1930 on spectra taken with a concave grating and without slit, it was surprising to find how little the lines resembled each other in their structural details or how little either resembled the H and K lines in the chromosphere. It is evident that these two lines cannot take their origin in the same atom, or at least, not in the same atom in the same state of ionization. In spite of many attempts, no further progress has been made in deciphering the source of coronium.

In the elucidation of the enigmas of the corona much substantial progress has been made but yet there are many unsolved problems. According to Rosseland, "the corona has stimulated speculation to the breaking point, it being even suggested that there we witness our recognized physical laws set at naught by nature itself." It is evident that the

[10] *Proc. Phys. Math. Japan, 19*, 693, 1937.
[11] *Mem. Czech. Astr. Assn.*, No. 5, 1937.
[12] *Nature, 139*, 584, 1937.

cause of the coronal radiation cannot be found in the emission spectrum of coronium which contributes only a very small fraction to the total energy of coronal light. We must therefor seek the explanation of the mysterious corona in the spectrum of the middle and outer corona.

As the result of many different theories, it is now generally recognized that the electron [13] must play an important role in explaining the radiation of the corona. If the continuous spectrum is caused by free electrons, then the average thermal velocity of the electron would cause a Doppler effect of the order of ten angstroms which would effectually wipe out all Fraunhofer lines with the exception of the broad wings of the H and K lines. Electron scattering will give an adequate explanation of the absence of dark lines in the inner corona and also is in conformity with the fact that there is little difference in energy distribution between the radiation of the corona and that of scattered sunlight.

Although substantial progress has been made there are important problems connected with the corona that await solution. In future eclipses there is a great need of carefully designed spectrographs of great speed and large dispersion. At the 1937 eclipse, Dunham had the most powerful spectrograph ever used at an eclipse for the chromosphere and for the corona. With smaller dispersions we need to compare the intensity distribution of coronal light with that of the photosphere and we need more and better spectra that will give the dark lines in the outer coronal spectrum that should be taken under clear skies devoid of water vapor in order that the photographs may verify that the Fraunhofer lines are coronal in origin and do not come from sunlight scattered in the earth's atmosphere.

Lyot has contributed much valuable information to the emission spectrum of coronium but the outstanding problems connected with the corona will be solved only by observations made at total eclipses. The chief difficulty is that on the average only one minute per year is given to any astronomer

[13] *Handbuch der Astrophysik*, 7, 404, 1936.

to secure his important observations. The next total eclipse to be observed will be on October 1, 1940, visible in Brazil and South Africa.

In recent years the chief contributions from observations made during total eclipses of the sun have had no connection with the corona but have been in two widely separated fields of investigation. In 1919, at the eclipse which was the forerunner in the Saros cycle of the eclipse of June 8, 1937, the whole world was electrified by the confirmation of the Einstein prediction that rays from stars are bent when under the influence of the strong gravitational pull of the sun. Although the splendid observations at the 1922 eclipse seemed to give a complete verification [14] of the Einstein value, it was found by Freundlich from observations at the 1929 eclipse that the deflection at the sun's limb was considerably greater than 1.75″ required by theory and amounted to 2.24″. The chief trouble with eclipse plates is that the same stars which furnish the relativity displacements must also give the scale of the photographs. The stars at the 1929 eclipse were badly distributed around the sun as is seen by the fact that out of a total of 18 stars used to measure the relativity effect there were 17 stars located on one side of the sun and only one star on the other side, with the result that scale value was poorly determined. A discussion by Trumpler of all observations at the eclipses of 1919, 1922 and 1929 gives the relativity deflection reduced to the sun's edge as $1.79'' \pm 0.06''$.

In view of the discordant results from the 1929 eclipse, it is evidently necessary to repeat the observations, devoting special care to a more accurate determination of the scale value of the photographs. Unfortunately, this obvious recommendation can hardly be put into effect for the reason that the star fields at total eclipses in the next twenty years do not contain a sufficient number of bright stars. In spite of these drawbacks, the British astronomers are planning to test again the Einstein deflection as a part of a very extensive program of observations for the 1940 eclipse.

[14] Ibid., p. 408.

The sun is the nearest of the fixed stars and it is the only star which permits a detailed study of conditions in the outlying atmosphere. As these conditions have gradually become understood in the sun they have furnished the method of interpreting many outstanding stellar problems. The greatest contribution in recent years from eclipse observations has been the rather perfect knowledge of heights to which the gases of the chromosphere extend above the surface of the sun. Photographs of the flash spectrum taken without a slit give a spectrum of curved arcs from which heights are immediately derived. From these heights furnished to Saha fifteen years ago he was able to compute the conditions of temperature and reduced pressure that cause an atom to lose an external electron and become ionized. His theory of ionization coupled with increased knowledge of the structure of the atom has made the past fifteen years the most prolific and most exciting years in the whole history of astronomy.

All the prominent lines in the spectrum of the sun have been assigned to multiplet groups with known excitation potentials measured in electron-volts, the arbritary intensity scale of Rowland has been submitted to calibration tests which have revealed that the intensities depend on the numbers of atoms engaged in the formation of the spectral lines. From the weakest iron lines perceptible in the solar spectrum to the strongest the number of atoms involved increases about one million times.

A person with no knowledge of the theory underlying multiplet groups would not advance very far in the practical operation of correlating heights from eclipse spectra with intensities before the fact would be forced upon his attention that the lines of greatest intensities generally reach the greatest heights, and moreover for any element considered, the intensities and heights are greatest for lines of lowest excitation potential.

Fascinating work on sun-spots was started [15] by Evershed and carried through to completion by St. John using the 150-

[15] *Eclipses of the Sun*, p. 340.

foot tower telescope of Mt. Wilson. With the slit of the spectrograph placed across the image of a spot it was found that the wave-lengths in the penumbra of the spots were different from the values at the center of the sun. These "Evershed displacements," as they are called affected practically all lines but the amount depended on the intensities of the lines. The heights derived from eclipse spectra furnished the explanation. The layers closest to the sun's surface have a motion of translation out of the spot at the rate of 2 km. per sec. At greater and greater heights this motion becomes less and less.

At a certain level the outward motion of vapors ceases and at still greater heights the motion of translation is in the opposite direction into the spot. At the maximum heights reached by the H and K lines of ionized calcium, the speed of motion into the spot is 3.8 km. per sec.

The Evershed effect shows that there is a circulation of gaseous material in the neighborhood of sun-spots, or in other words, that each sun-spot is a great vortex.

One consequence of the Einstein theory of relativity is that wave-lengths in the sun must be greater than in the laboratory, the amount of shift to the red depending on the wave-length. The best element for determining the red shift [16] in the sun is iron. From the discussion of abundant material together with heights derived from eclipse spectra, it is evident that the relativity shift is most certainly present, but in addition there are systematic differences between the effects shown by the strong and the weak iron lines, the strong lines being those that extend to the greater heights in the sun. The systematic differences are most readily explained as a Doppler shift caused by a circulation of vapors in the sun's atmosphere. On account on the higher temperatures and higher pressures near the photosphere, the solar activity causes the Fe atoms to ascend through the medium of thousands of weak lines of high excitation potentials. In the upper reaches of the chromosphere under reduced pressures

[16] Ibid., p. 416.

some of the atoms lose an external electron and become ionized. Later the atoms descend from their maximum heights and in their descent some of the atoms gain an external electron and again become neutral. According to this interpretation, in a direct photograph of the sun showing a mottled surface, the atoms ascending exhibit themselves over the small bright granules and the descending ones over the larger dark interspaces, the observed effect being an integrated one.

In the celestial laboratory in the sun the astronomer has available for his researches very high temperatures and very minute pressures far transcending the conditions available to the physicist in terrestrial laboratories. As the result of a knowledge of the structure of the atom, Henry Norris Russell and others have assigned practically all the strong lines in the solar spectrum to multiplet groups and have ascertained the energy, measured in electron-volts, necessary to produce the spectral lines. From a knowledge of the multiplet intensity formulae it has been possible to learn the relative numbers of atoms involved in the transfer from one energy level to another in the production of absorption or emission in the sun's atmosphere.

The heights determined from eclipse spectra to which the solar gases extend in the chromosphere have permitted many other important correlations. These correlations are shown by the dozen lines of a multiplet involving [17] strong Fe lines, where 100 times as many atoms are active in producing the strongest line in the multiplet at wave-length 3820 A as go to form the weakest line at 3940 A. Even within a single multiplet, the heights found directly from the flash spectrum or from the Evershed effect in sun-spots, are not constant in amount but the heights are greatest for the lines of greatest intensities which involve the largest number of atoms.

Again, a knowledge of the heights from the flash spectrum to which the various lines in multiplets are observed combined with information from the number of atoms involved gives

[17] Ibid., p. 348.

important knowledge [18] regarding the density distribution of various elements in the lower chromosphere. The results seem to indicate that turbulance and not selective radiation pressure is responsible for keeping the different elements so well mixed.

From the flash spectrum there has been determined [18] the relative abundances of various elements and the results from the chromosphere have been compared with the abundances found in the sun's reversing layer, in stellar atmospheres, in the earth's crust and in stony meteorites. It is shown that within errors of observation the compositions of all samples are alike with the exception that hydrogen is conspicuously deficient in the earth's crust and in meteorites and is probably much more abundant in the chromosphere than in the reversing layer.

The eclipse of 1936 was observed throughout Siberia and Japan with much interference from clouds while the 1937 eclipse had clear skies everywhere. In 1936, the British expedition in Japan under the direction of Stratton obtained excellent results in spite of many clouds, while in Siberia the Harvard-Massachusetts Institute of Technology expedition under the guidance of Menzel secured carefully standardized and calibrated spectra with large dispersion in the spectral region 3200–10000 angstroms. In Siberia, Williams found 12 new lines in the chromosphere in the infra-red with wavelengths between 9000 and 10049 A. He also photographed eight lines of the Paschen series of hydrogen. At the 1937 eclipse on Canton Island, Dunham secured spectra of the chromosphere and corona with large dispersion and jumping-film. Mitchell obtained spectra with three concave gratings without slit. In the infra-red with the Allegheny grating he photographed the strong calcium triplet and also 25 lines of the Paschen series of hydrogen. Also he photographed the strong coronium line at 7892 A, the line discovered in 1925 by Curtis and Burns using the same grating.

[18] *Astrophysical Journal*, 77, 1, 1933.

At the 1936 eclipse from spectra taken outside of totality, Royds [19] of the British expedition found that the red shift at the sun's limb is the same in amount inside and outside an eclipse, thus ruling out the possibility that scattering in the earth's atmosphere is the explanation why strong and weak spectral lines show different relativity shifts. In the same expedition, Thackeray [20] finds that the intensities of lines in the Balmer series of hydrogen are in close agreement with the theory of Menzel that the hydrogen lines owe their intensities to some form of excitation in addition to straight electron capture. From the 1937 eclipse Mitchell finds a similar agreement for the intensities of the Paschen series of hydrogen.

Menzel [21] and others at Harvard have published valuable results from the 1932 spectra. Information is given about multiplet intensities and density gradients, self-reversal of hydrogen lines and the general theory of curves of growth and self-reversal of emission lines. Mitchell has nearly ready for publication a discussion of the October 21, 1930 eclipse which will give a redetermination of heights and the density distribution of gases in the chromosphere. This discussion was aided by a grant from the Penrose Fund.

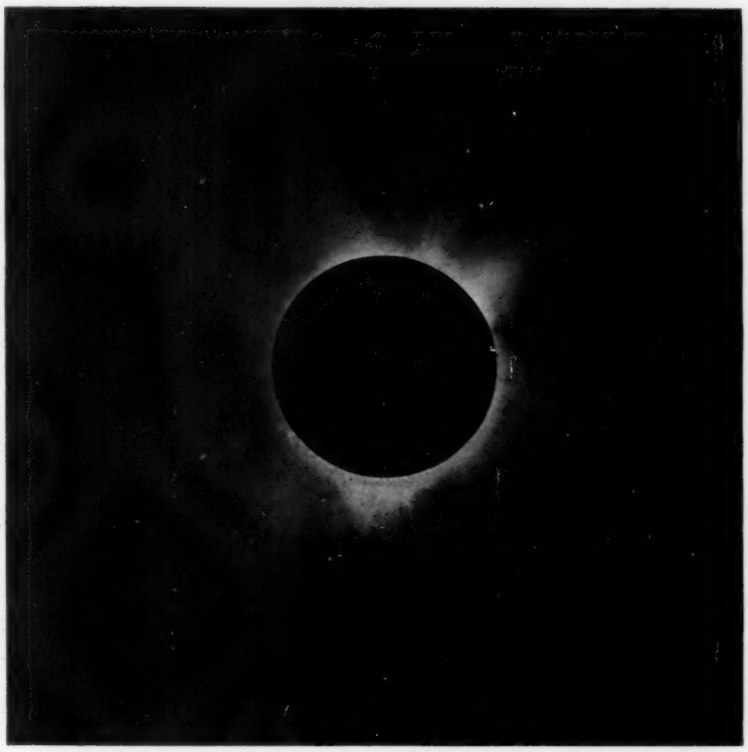
On account of the great activity in recent years of eclipse observers, particularly the American and British, most of the problems have been cleared up that might be attempted by interested amateurs with moderate equipment. There are many mysteries still connected with the corona, with the chromosphere and with the relativity displacements. For instance, from spectra taken at the 1936 eclipse it was found by Thackeray [20] that the emission lines of neutral metals show an unexplained displacement to the violet relative to limb absorption. The emission lines in the chromosphere, if secured with excellent definition can give valuable information about line profiles which in turn will give further informa-

[19] *Monthly Notices*, *97*, 692, 1937.
[20] Ibid., 672, 1937.
[21] Harvard Circ., 410, 1935.

tion about the structure of the atom and the mechanism whereby lines in spectra are produced.

In planning for future eclipses, the British have a great advantage over us in the United States for the reason that our British cousins see each other very frequently at the monthly meetings of the Royal Astronomical Society. With us in the United States eclipse expeditions become possible only when some interested person is able to raise the rather large amount of money necessary, while in Great Britain the expeditions are handled more efficiently by the Joint Permanent Eclipse Committee organized under the auspices of the Royal Society and Royal Astronomical Society.

PLATE I



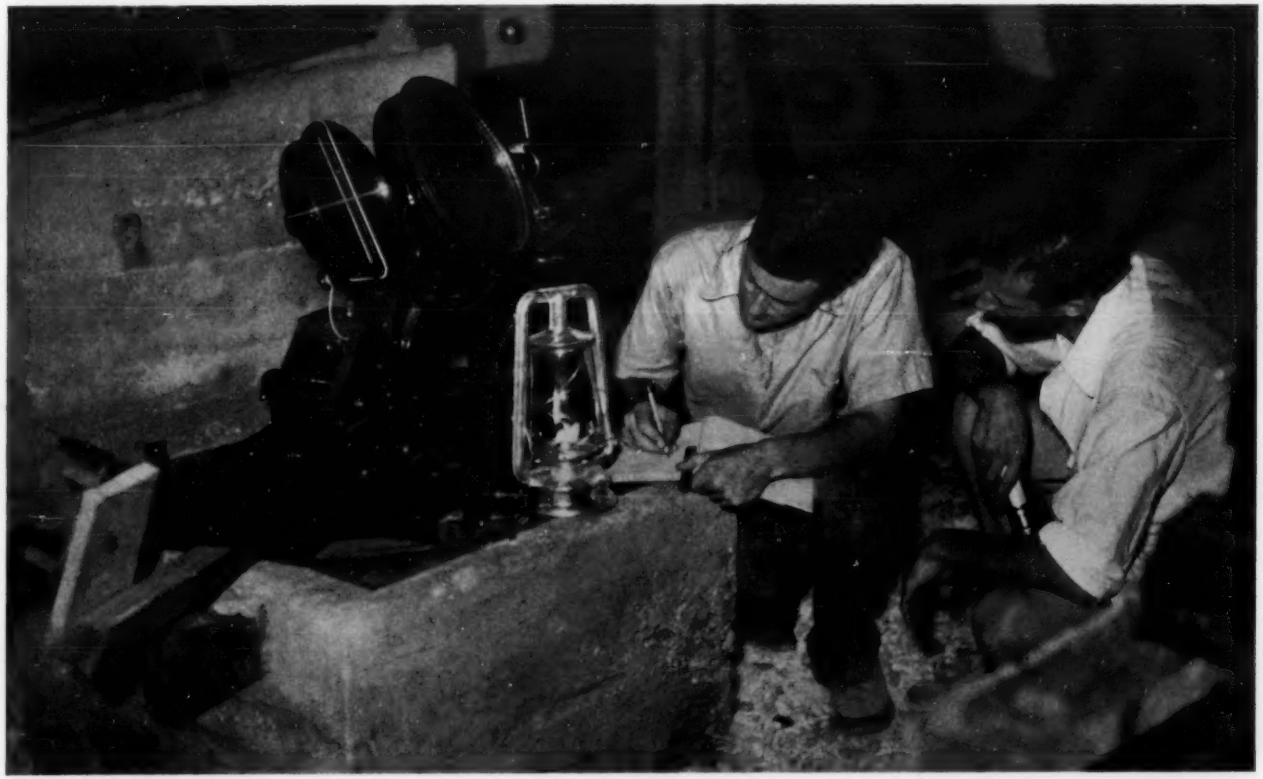
The total solar eclipse of June 8, 1937, made with 10 seconds exposure by Dr. Irvine C. Gardner of the National Bureau of Standards, a member of the National Geographic Society–United States Navy Eclipse Expedition to Canton Island in the mid-Pacific. This photograph was taken with a camera designed by Dr. Gardner and built at the National Bureau of Standards. It has a nine-inch astrographic lens composed of four separate components.

PLATE II

THE 1937 CORONA

photographed with a camera of six inches aperture and fifteen feet focal length by John E. Willis, of the U. S. Naval Observatory, a member of the National Geographic Society–United States Navy Expedition. Time of exposure 24 seconds.

PLATE III



ADJUSTING THE MOUNT WILSON SPECTROGRAPH

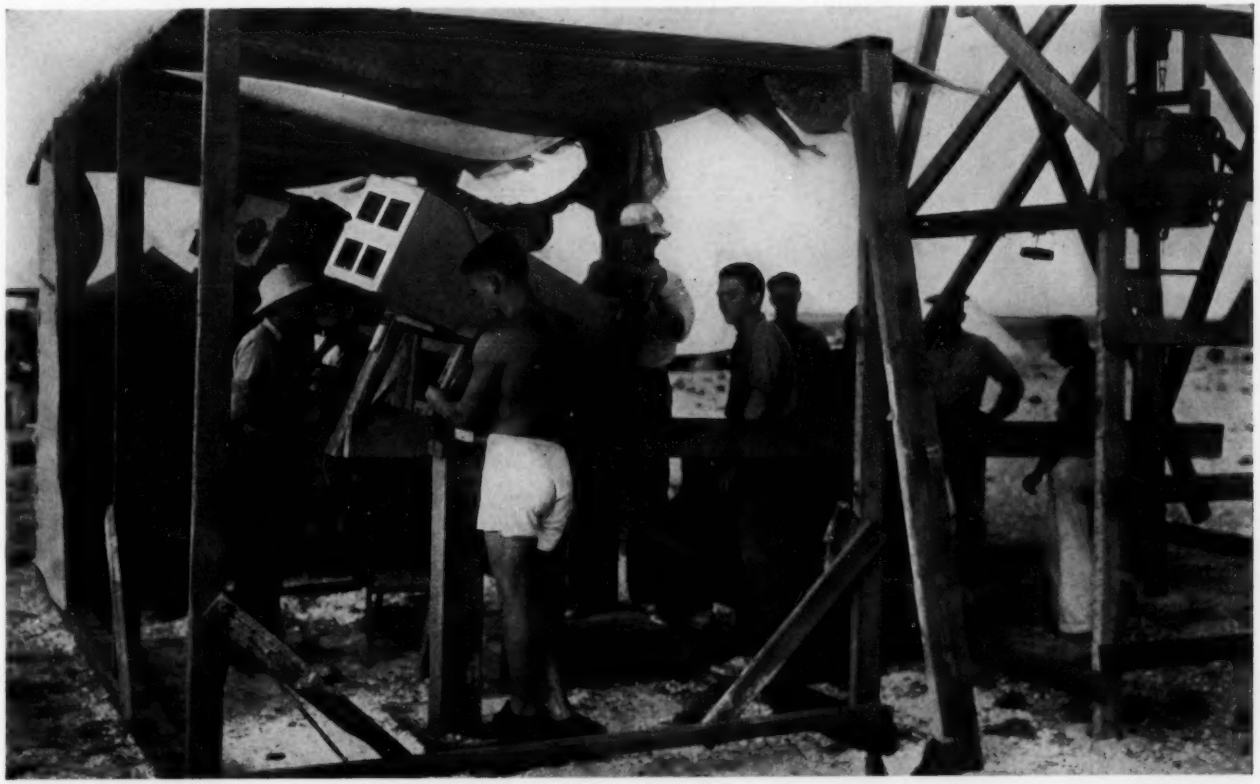
Theodore Dunham and C. G. Thompson burning the midnight oil working on the most powerful spectrograph ever at an eclipse. (The instrument is virtually a duplicate of that attached to the 100-inch reflector at Mt. Wilson.)

PLATE IV

THE ECLIPSE CAMP ON CANTON ISLAND

The bell-shaped tents are those of the New Zealand expedition, the balance those of the National Geographic Society–United States Navy party.

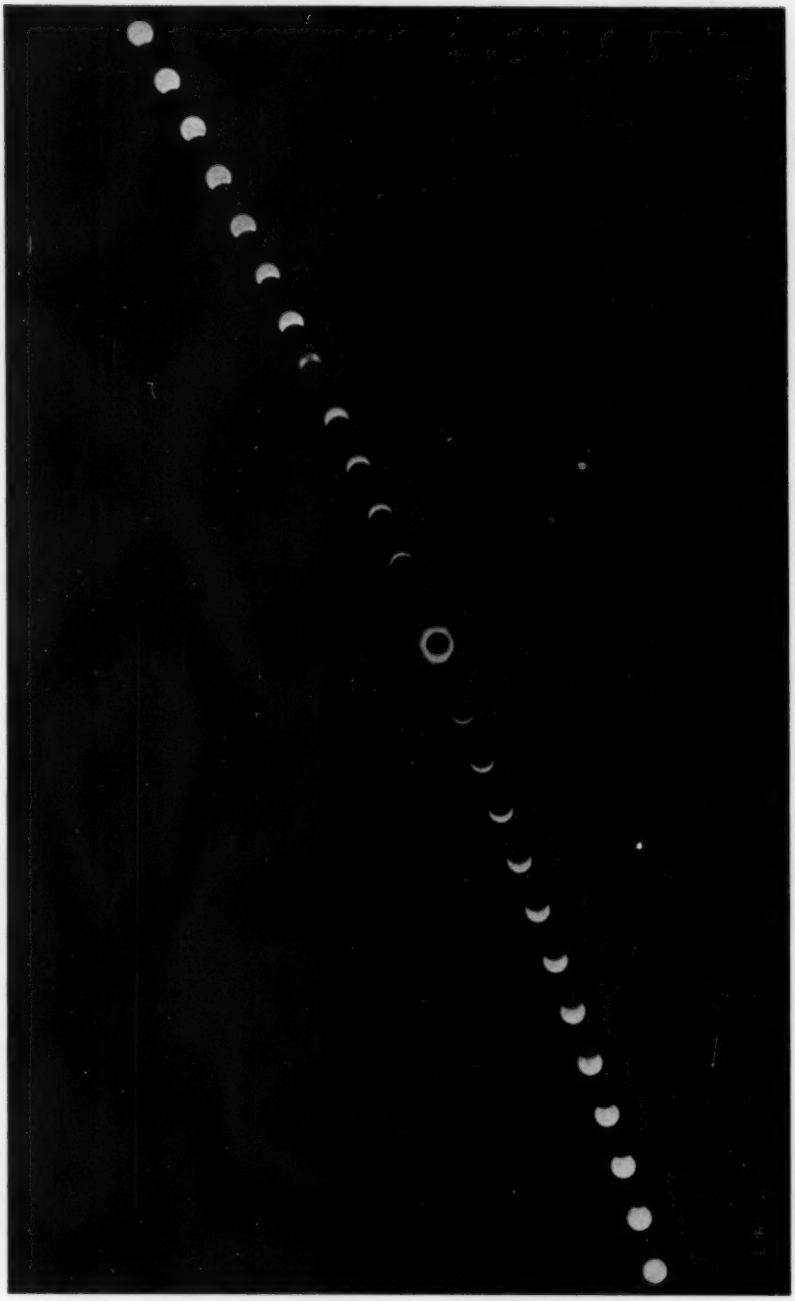
PLATE V



F. K. Richtmyer and his Crew on Canton Island

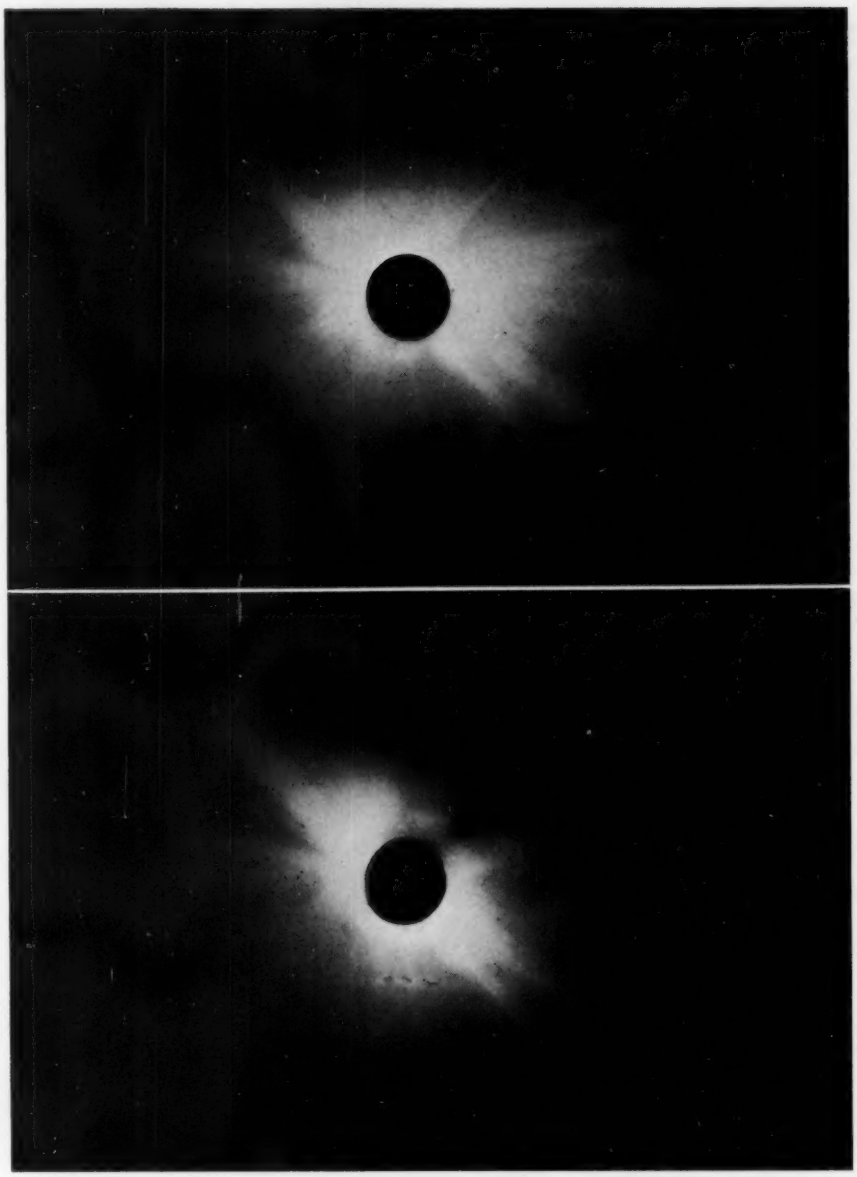
The polar axis and clock were constructed by H. D. Curtis.

PLATE VI



THE PROGRESS OF THE 1937 ECLIPSE
made with a stationary camera with 25 exposures lasting for more than two hours from first contact (bottom) to fourth contact (top) with the total eclipse at the middle. Note that only one of the exposures, the fifth after totality was interrupted by thin clouds.

PLATE VII



THE POLARIZATION OF CORONAL LIGHT

Photographs made by F. K. Richtmyer with identical lenses and exposures but through polaroid screens turned at different angles.

VITRIFICATION AND CRYSTALLIZATION OF PROTOPHYTA AT LOW TEMPERATURES

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(Communicated by Detlev W. Bronk)

ABSTRACT

I. *Theoretical Considerations.*

The conditions which determine whether a homogeneous liquid will solidify into the crystalline or into the vitreous state are discussed and a brief account of the rôle of the speed of crystallization, V, and that of grain formation, N, is given.

These concepts are applied to the causes of the death due to crystallization of non-dehydrated organic cells, and the physical conditions of cooling are developed under which such organisms can be solidified with a minimum of crystallization, assuring to this extent the preservation of their potential life.

II. *Experimental Procedure.*

A method of staining cells of certain types (yeasts) which is indicative of their living or dead condition is described. Thus the death rate, δ, for exposure to low temperatures under different conditions, can be determined by direct count.

Different techniques of exposure favoring and preventing crystallization are developed.

III. *Experimental Results.*

The rate of cooling of suspensions of living cells in aqueous media was varied from 1 to 10^4 deg. sec^{-1} and thus conditions favoring either crystallization or vitrification of the cell plasma were produced. The death rate at an exposure of − 185° C. was thus varied from approximately 75 per cent to approximately 3 per cent.

The duration of exposure at this temperature was varied from 5′ to 6000′; no appreciable influence of the time of exposure was found.

The temperature to which the cells were cooled was varied from the boiling point of liquid air (− ° C.) to the boiling point of liquid hydrogen (− 252° C.) without appreciable influence on the death rate. Indications, however, were found that at − 50° C. the duration of exposure affects the death rate.

Repeated exposures of the same cells under conditions not especially favoring vitrification were studied and the integral death rate, δ_n (death rate after nth exposure) was investigated with respect to a constancy of the death rate (δ) at each freezing. It appears that the death rate for repeated freezings is not controlled by simple probability relations, but that other effects enter such as weakening of cells by previous exposures and the selection of cold-resistant cells by survival.

I. THEORETICAL CONSIDERATIONS

(*a*) *Crystallization and Vitrification*

THE transition from the liquid into the solid state by lowering the temperature of the system can occur in two ways.

In one case the liquid phase changes into the crystalline; the act of crystallization is then characterized by the emission of heat of fusion. Thermodynamically speaking, the crystallization reduces the entropy of the system as well as the heat content $\left(\int_0^T QdT\right)$, the free energy is a minimum (strictly true only for an ideal crystal) and the state is thus the most stable for this particular temperature; and, in general, in the absence of allotropy, the most stable down to the lower end of the temperature scale.

Experience has shown, however, that in spite of the high stability of the crystalline state, crystallization does not necessarily take place. In other words, the actual occurrence of crystallization depends on a number of statistical conditions which, if not fulfilled in the experiment, prevent the liquid from the attainment of a state of larger stability (higher degree of order). In such cases the system remains a "liquid" for the rest of the temperature scale. Needless to say, the word "liquid" refers in this connection to the statistical arrangement of the molecules of the system, in contradistinction to the regularity of the arrangement in a crystal. It does not refer to a number of other physical qualities such as viscosity, etc. In fact, the viscosity soon increases with decreasing temperature to such an extent that the substance is physically solid but not a crystal. This state is generally called the glassy or vitreous state.* It is physically characterized by a continuous increase of the viscosity as the temperature is

* The following scheme is rather descriptive of the state of a system, representing all possible combinations:

1 Solid	2 Liquid
a Crystal (anisotropic)	*b* Glass (isotropic)

1*a* is a crystalline solid; 2*b* a normal monomolecular liquid; 1*b* a vitreous solid; 2*a* a crystalline liquid (certain organic liquids and probably the second modification of liquid helium). A glass would thus be represented by 1*b*.

lowered, by a remaining isotropy and by the absence of any discontinuity at the melting temperature; thermodynamically it is characterized by a higher entropy as well as a higher heat content, the latter resulting from the fact that the heat of fusion remains within the system. (Fig. 1.)

The factors that determine which of the two states will occur when the liquid is brought below its melting temperature are several, and, as they have important bearings on the problem, will be briefly outlined.

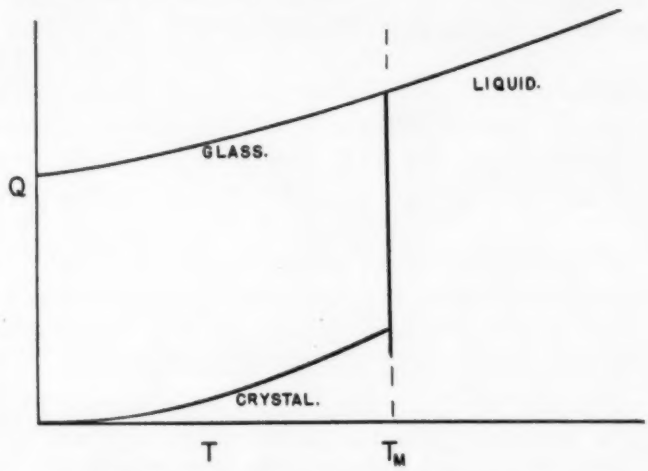

FIG. 1. $Q(T)$—Diagram for the liquid, vitreous and crystalline state of a substance in general. (T_m = temperature of fusion.)

Tammann [1] gave the first systematic description of the tendency of a supercooled liquid to form "grains," *i.e.*, nuclear centers of crystallization within itself, and defined the number N of such centers originating at constant temperature per unit time in a unit volume as a characteristic constant for a particular substance. He also showed that N depends upon the temperature in a way as shown qualitatively in Fig. 2*a*; at T_m (melting temperature) N is zero, but increases rapidly to a maximum value at a temperature in the vicinity of T_m. It then decreases again and disappears at lower temperatures. The reason for this latter behavior is obviously due to the

rapidly decreasing mobility of the molecules at lower temperatures, so that one would expect the decline to be proportional to the increase of the viscosity. Aside from this, N depends in the most general way upon the probability of the formation of centers within the system. This probability is obviously controlled by the following factors: the number of molecules within the system, the mobility of the molecules within the liquid and the configuration of the crystal lattice characteristic for the particular liquid. Qualitatively speaking, it can be said that the smaller the symmetry of the crystal lattice and the farther the molecular configuration of the lattice is re-

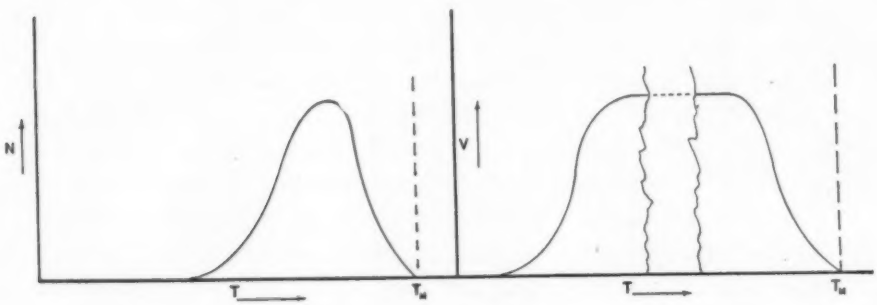

FIG. 2*a*. $N(T)$—Diagram representing the variation of the formation of crystallization centers with temperature.

FIG. 2*b*. $V(T)$—Diagram representing the variation of the linear rate of crystallization with temperature. In the omitted temperature interval V remains constant.

moved from that of close packing, the less is the probability of the formation of a nucleus.[2] Finally, certain other factors have to be considered, such as molecular association at temperatures slightly above the melting point (H_2O) as well as the presence of surfaces within the liquid such as colloids and suspended particles in general.

The second main factor is the linear rate of crystallization V, which characterizes a liquid in a way similar to N and has also a qualitatively similar dependence on the temperature; as shown in Fig. 2*b*, it rises from zero to $T \leq T_m$, attains a maximum, remains constant for a certain interval and then

declines towards zero. The causes of this behavior are somewhat analogous to, although more complex than, those for N.

The difference between a substance with a high N and a small V and one with a small N for a large V is that under the same cooling conditions a polycrystalline conglomerate would result in the former, and an oligo- or even single-crystalline habitus in the latter case. One can thus predict that liquids with small N and small V should have a tendency to vitrify, provided they are cooled sufficiently *rapidly* to a temperature where N and V are negligible.

One result of these considerations is that it should be possible to obtain every substance in either a vitreous or a crystalline state, provided that the proper conditions are maintained during the critical interval of temperature where N and V have large values. This postulate, however, has been verified for only a limited number of substances.

(*b*) *The Rôle of Crystallization in the Cold Death of Cells*

The irreversible changes which organic matter undergoes when exposed to low temperatures are of so involved a nature that even for the limited field of this paper certain general remarks have to be made.

From the highly diversified literature on the subject of exposing live matter to temperatures below room temperature [3] it is evident that there exists a form of cold death which may be termed "*constitutional*" (*CD*). It occurs if the organism is kept below its normal temperature longer than its energy supply lasts for counterbalancing the adverse effects of the cold. It is obvious that this *CD* can occur at any subnormal temperature before freezing, but the time factor as well as the type of organism will have a large influence upon the effects observed. The phenomenon of *CD* is outside the scope of this paper.

The other type of destruction of organic matter due to low temperatures is that caused by the crystallization of the cell plasma or liquids surrounding the cell, causing either a rupture of the cell membranes or some form of irreversible disorder

within the cell so that after thawing, the cell does not resume its activities characteristic for the live state. This phenomenon shall henceforth be called "*physical*" cold death (*PD*). When death occurs below the freezing point this *PD* as the only death cause is apparently restricted to the simplest forms of life, whereas, for higher forms it is often not possible to decide whether *CD* or *PD* caused the destruction. The numerous reports and experiments which claim the occurrence of "supercooling" in live matter fall into this realm. It has been described by several authors [4] that live cells have the tendency to supercool, that is, that the cell content remains liquid although the temperature stays below zero. If the temperature is reversed *before* crystallization starts, the process proves to be reversible, *i.e.*, the cell remains alive. Very early similar phenomena have been found by Bachmetiew [5] on fairly high forms of organic matter, such as larvae of insects, which proved to be able to supercool over fairly large ranges down to − 15° where crystallization suddenly started, causing the death of the organism. The well known freezing curve Fig. 3, demonstrates that the irreversibility sets in with the beginning of spontaneous crystallization, indicated by the sudden rise of temperature from the minimum temperature to the freezing point, due to the display of the heat of crystalliza-

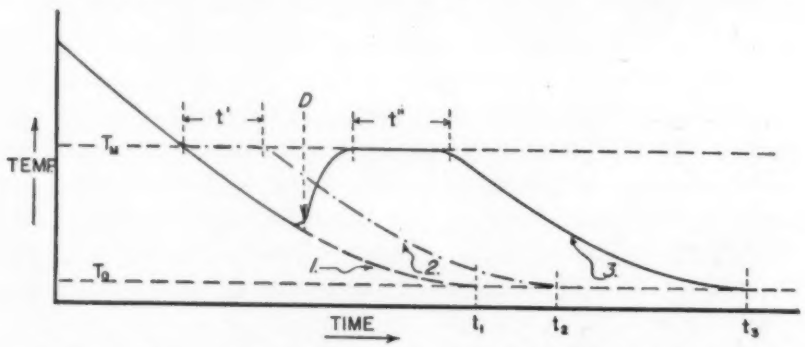

FIG. 3. Temperature-time function for (1) continuously cooling substance; (2) substance crystallizing at T_m, remaining at T_m for the time t'; (3) substance super-cooling down to D. Due to the emission of the heat of crystallization T rises to T_m for the time t''. Type (3) is characteristic for organic substance. Death sets in at D.

tion. As soon as the organism is made to display this heat, death occurs. There seems to be no doubt that this death is *PD*, although it is not easily decided whether the tendencies towards supercooling characteristic for live organisms are of a constitutional or a physical nature.

It is, however, definitely proven that there are numerous species of organisms to which the *CD* does not apply, as shown by the experiments on thoroughly dehydrated protozoan [6] and even metazoan [7] forms which withstood, without particular precautions, temperatures down to the boiling point of He (4.2° K.). In these cases the *PD* was avoided by complete dehydration so that no substance for crystallization was present. Experiments of this type are of course restricted to seeds, spores and infusoria, and it is also obvious that neither the time of exposure nor the temperature has any influence.

The question arising from these known phenomena is: is it possible to expose *non-dehydrated* organisms to low temperatures without destroying their potential life? It is known for a number of bacteria, moulds and fungi which contain only very small quantities of liquids, that they survive very low temperatures without dehydration, whereas, it is also known that the majority of protozoa and plants cannot be cooled without the destruction of life.

(*c*) *Conditions of Vitrification*

It has been stated above that the *PD* consists in the crystallization of the cell plasma. This leads to the assumption that if conditions under which the organism is cooled can be so arranged that vitrification is more probable than crystallization, the *PD* should be avoided. In contradistinction to crystallization, vitrification would not disturb the molecular arrangement of the cell contents, and also, the more macroscopic effect of crystallization, the lesion of the cell membrane, is impossible.

The greatest handicap in experiments connected with the vitrification of water and diluted aqueous solutions is the un-

usually high value of N and V (see I, *a*) which render any attempts at the vitrification of organisms very difficult.

The fundamental conditions to be fulfilled for obtaining the vitreous state are the following ones:

1. Since for a given value of N the probability for the formation of a nucleus in a given system depends upon the *number of molecules available*, the system should be chosen as *small* as possible, *i.e.*, to contain a minimum of molecules. The minimum size of a system for organic matter is a single cell; among different cells the smallest should have the largest probability of vitrification.

2. For a given N and a given number of molecules, the probability of the occurrence of a nucleus depends upon the temperature (in the way shown in Fig. 2*a*) and the time during which the system is at a certain temperature. As N decreases exponentially towards low temperatures after the maximum, it is very important to *cool* the system *as quickly as possible* over the temperature interval in which N has large values. Below this "critical" temperature interval, the cooling rate is of no importance as far as N is concerned. From this it is obvious that the ultimate temperature to which the system is cooled is irrelevant as long as it lies below the "critical" temperature range.

3. It has been stated above that N depends upon the complexity of the molecular configuration in the crystal. As it is not possible to change the chemical constitution of the aqueous solution within and around the organism without damage to the latter, the only way consists in increasing the surface exposed to the liquid; this can be accomplished by the addition of complex colloidal matter causing, due to the increase of molecular complexity, a decrease of N. Such substances (hydrosoles of organic substances) are gelatine, agars, glues, the addition of which does not affect the organism.

4. The spontaneous rate of crystallization depends upon almost the same physical factors as N; accordingly, the conditions for vitrification with respect to N hold also for V, with

the possible exception of 3, in which latter case the addition of colloidal matter reduces V in certain cases.

It is generally to be assumed that V is a much more critical factor than N for the avoidance of PD.* In order to control V, it is necessary to consider the critical temperature interval for V just as accurately as for N. Another point to be considered with respect to V is the *time of exposure* to low temperatures. As V, for thermodynamic reasons, will become zero only at zero temperature, it is to be expected that a gradual, though slow, crystallization out of the statistical arrangement of the glass will go on with time. One should therefore expect that the time of exposure will have an effect upon the probability of the survival of the organism unless the temperature is chosen so low that the value of V is sufficiently small to be negligible for time intervals on a terrestial scale.

5. If it is desired to warm the vitrified substance while preserving the capacity for life, condition 2 has to be fulfilled in *warming as well as in cooling* in order to avoid crystallization when the substance passes the critical temperature interval in the direction towards room temperature. If the final temperature be chosen sufficiently low (that of liquid air) the warming process is not as critical as the cooling process in this particular regard, because the critical temperature interval for aqueous solutions ends at approximately $-110°$ C.[8] so that an interval of 70° is available for the time of transfer from one bath to the other, where the warming rate cannot be forced.

* It can be taken as a generally accepted fact that a perfect crystal is just as well an ideal abstraction as a perfect glass. The X-ray spectroscopic evidence has not yet proved the existence of either of the ideal states. In the case of the vitreous substances, even fused quartz has been proven to contain small realms of crystalline molecular array, the dimensions of which are far below the limit of optical detectability. To speak of the vitreous state of a cell is thus an inaccuracy and means that a cell is "vitrified" if the size of the crystalline realms are smaller than dimensions fatal for the particular cell. This relation may serve to explain that organisms of different types but of approximately the same dimensions and structure cooled under identical conditions do not survive with the same probability. The size of the realms is determined by V, whereas N determines their concentration.

II. Experimental Procedure

(*a*) *Determination of the Death Rate* δ

A quantitative experimental study of the *PD* or organic matter requires, besides an adequate experimental technique for the production of the conditions of vitrification, the establishment of a definite criterion of the live or dead state of the cell. The most obvious way is the growth of cultures of cells treated, if the organisms represent bacteria, fungi or protozoa easily cultivated. This technique has been used generally in the work quoted,[6] also in the first experiments of the authors. It is, however, not indicative of the actual death rate because of the purely qualitative indication of the existence of some or even *one* surviving cell. Furthermore, such experiments which require absolute sterility in order to obtain positive results, seem offhand undesirable and to be chosen only if no other method is possible.

Another way, which has the advantage of measuring directly the death rate δ, that is, the ratio of dead cells to the total number of cells present, is the use of motile protozoa where the sample can be counted before and after exposure; δ is then established by the count of the still and the moving organisms. This method also was tried extensively. It turned out, however, that it is often very difficult to distinguish between a dead organism and one in a state of rest, which state most motile forms can enter.

It was therefore considered most fortunate that a method of differential staining was found applicable which permits visual distinction between dead and live cells. Experiments of the authors on a large variety of organisms showed that the use of colloidal stains is not necessarily indicative for *PD*, but that stains of the type of methylene blue, which are not in general considered colloidal,* can serve as very reliable indi-

* It is known that a number of stains, most of them, however, of a colloidal character, such as congo red and trypan blue, do not affect certain organisms while they are alive, and that the dye can stay in contact with the cells for a considerable length of time if not too concentrated. Seyderhelm and collaborators,[9] used a mixture of congo red and trypan blue known as "Seyderhelm stain." It was applied first to leucocytes as a differential stain and also to protozoa.[10]

cators if a number of precautions are taken. Such dyes have lethal effects if too concentrated and it is necessary to find for each type of organism the proper concentration, which latter obviously consists in a compromise between a concentration high enough to give a well stained effect upon the dead cell, though at the same time small enough not to have lethal effects within the time of observation.

This being once accomplished, the technique becomes very simple and seemingly more reliable than any other method if a number of technical precautions are observed. The death rate δ is determined simply by placing a drop of the treated culture containing the dye on a slide under the microscope and taking one or several photographs of it. An enlargement of the photograph is made and the number of stained and unstained cells counted.

Two conditions have to be fulfilled here. It is important to filter the illuminating light in order to produce a maximum contrast between the stained and unstained cells, *i.e.*, the light should contain chiefly the colors complementary to those of the dye used. This condition being very important, it renders it necessary to find, either by trial and error, or by spectroscopic methods, the proper filter combination for each dye. Furthermore, the sensitivity of the photographic emulsion has to be chosen properly. The photomicrographs show thus white and black cells which can easily be counted.

Secondly, it is important to choose the magnification and the concentration of the preparation in such a manner that a large number of cells (several hundred to several thousand) are on one photograph, in order to obtain fair statistical percentages. The errors connected with this method are small and can occur only if more than one layer of cells is in the preparation, as if several cells lie on top of each other, the effect of a dark cell may be created. This can be avoided by applying the following technique: the carefully cleaned slide is slightly heated and then covered with a thin layer of cleared gelatine solution. The slide is then allowed to cool and dry until the gelatine is almost completely hardened. A concen-

trated drop of the culture containing the stain is then put upon the gelatine coat and distributed over the latter by rubbing under very gentle pressure with a fingertip until all of the gelatine is equally wetted. A wetted cover-glass is then slipped over the slide and the surplus liquid soaked up with filter paper. Investigation has shown that δ is not increased by this manipulation. Under low magnification one finds large regions on the preparation consisting of 10^3 to 10^4 individuals in monocellular arrangement. Counts within these realms prove to be very reliable.

Another precaution has to be taken if the organisms are sensitive to blue light or to heat. In this case they are destroyed during the time of observation by the illuminating light source. In such cases it is possible to observe a rapid increase of stained cells within a short time during observation of the slide. This renders it necessary to use effective heat filters and also to shorten as much as possible the time of exposure to the light.

(*b*) *Techniques of Vitrification*

In the following, a number of different techniques will be briefly described which have been employed by the authors in order to realize experimentally the conditions of vitrification discussed in (I*c*).

Condition 1 calls for small systems of molecules, which means either the isolation of the single cell as the minimum size of a system or, where the cells are small enough, the use of small droplets or thin layers (membranes). This has been done in cases where bacteria were used by spraying liquid cultures through nozzles similar to atomizers. This method is applicable only where the organism is small and has sufficient mechanical resistance to stand the shear to which it is subjected when passing the nozzle at the speeds necessary to atomize the more or less viscous liquid.

If the cells are too delicate, it is possible to resort to a technique which produces a thin membrane of the cell-containing culture medium in a large platinum loop. In this

case the largest surface possible for a given volume can be created in a relatively simple manner.

Condition 2 requires a high rate of cooling over the critical temperature interval, which is not as easily realized. It is obvious that the culture medium should be brought in contact with the low temperature bath as quickly as possible, and also that this bath should have a temperature as low as conveniently possible, *e.g.*, that of liquid air (90° K.). This can be done by shooting the spray under pressure into the cooling bath, or by dropping the membrane with its holder as quickly as possible into it. Experiments prove, however, that all such efforts to overcome V with the cooling rate are rendered futile by the "Leidenfrost phenomenon" which consists of the formation of an insulating gas layer around the warm object, rendering a quick temperature exchange impossible. It is obvious that this phenomenon will always occur when the *boiling point of the cooling medium lies below room temperature*, *i.e.*, the temperature of the medium at its introduction. As the cooling bath has to have a very low temperature, only very few substances for such cooling purposes are available (as they must also be liquid at low temperature).

Another condition has to be imposed upon this substance which is also imperative—it must be insoluble in water. The only substance which was found to fulfill these conditions is iso-pentane (C_5H_{12}) with a boiling point at 28° C. and a melting point at − 159.6° C.[11] * As iso-pentane has, like most other substances of very low crystallization temperatures, small values for N and V, it can be undercooled to the boiling point of N_2, where it remains a clear liquid of syrupy consistency. Fig. 4 shows the arrangement where a container with iso-pentane is submerged into a Dewar vessel containing liquid air. The temperature of the pentane is easily measured with a thermo-couple or pentane thermometer.

The realization of condition 5 is also easily obtained with the use of volatile hydrocarbons; the substance is transferred

* The use of isopentane for such experiments is only possible since it does not affect the cells as a solvent nor by any observable chemical reaction.

as rapidly as possible from the pentane bath into a bath of petrol-ether at room temperature and is moved vigorously in the latter, in order to further a rapid temperature exchange.

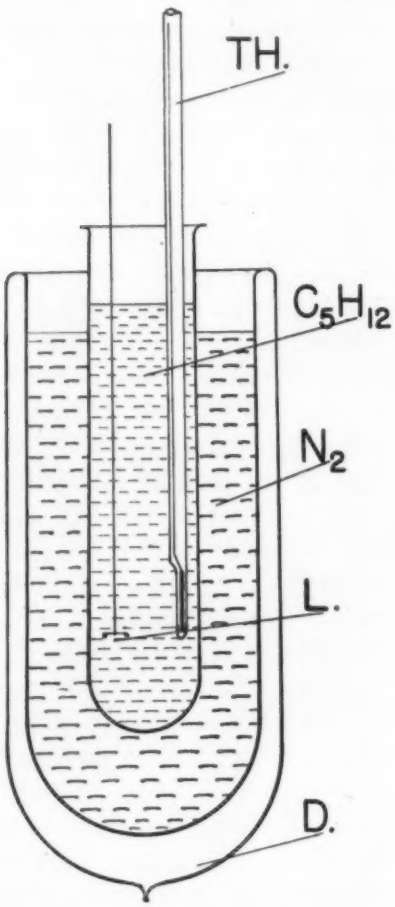

FIG. 4. Scheme of cooling apparatus. *D*, Dewar-vessel containing liquid nitrogen (N_2); suspended in it a container with iso-pentane (C_5H_{12}); *Th*, pentane thermometer; *L*, submerged platinum loop holding culture membrane.

Condition 3 may find its fulfillment in a number of ways. The introduction of gelatine has been used fairly early in the work, and observations similar to those of Luyet [12] have been made.

The process of vitrifying thus goes on as follows. The liquid culture medium (cf. below) is introduced into the spraying mechanism and sprayed into the pentane bath cooled with liquid air; or the culture medium is brought into the loop, the drop being thinned to the point where the membrane becomes almost unstable. Then the membrane is dipped as rapidly as possible into the pentane bath. For warming, the

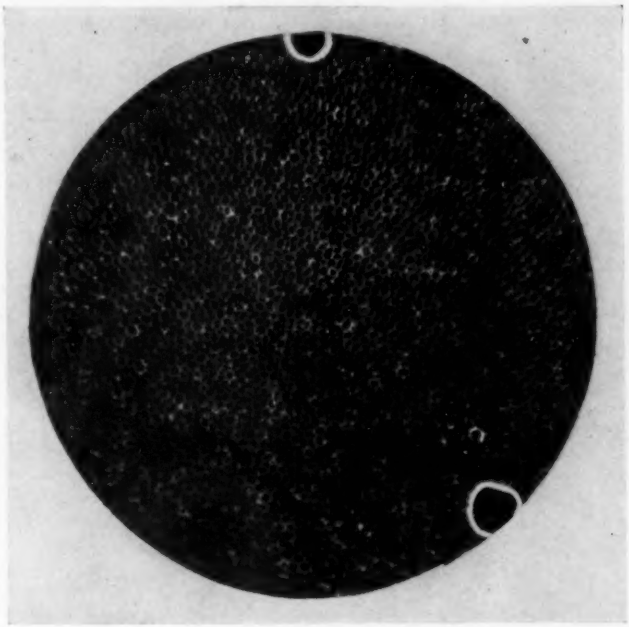

FIG. 5. Ultramicroscopic photograph of monocellular layer of *Saccharomyces cerevisiae* (cardioid condenser, obj. apochr. 20 ×, magn. 250 ×, Agfa Plenachrome).

solid contents of the pentane bath are transferred as rapidly as possible into the ether bath at room temperature. They are then placed upon a microscopic slide, the indicator stain is added in the proper concentration (cf. II*a*) and after mixing both thoroughly, the monocellular layer is formed on the slide, sections of which are then photographed under the microscope (Fig. 6–8). The photographs are then enlarged and the

number of black and white cells determined by counting, from which numbers δ is determined.

(*c*) *The Use of Yeast* (Saccharomyces Cerevisiae)

With regard to the considerations of II*a*, it seemed advisable to choose organisms for the low temperature experiments

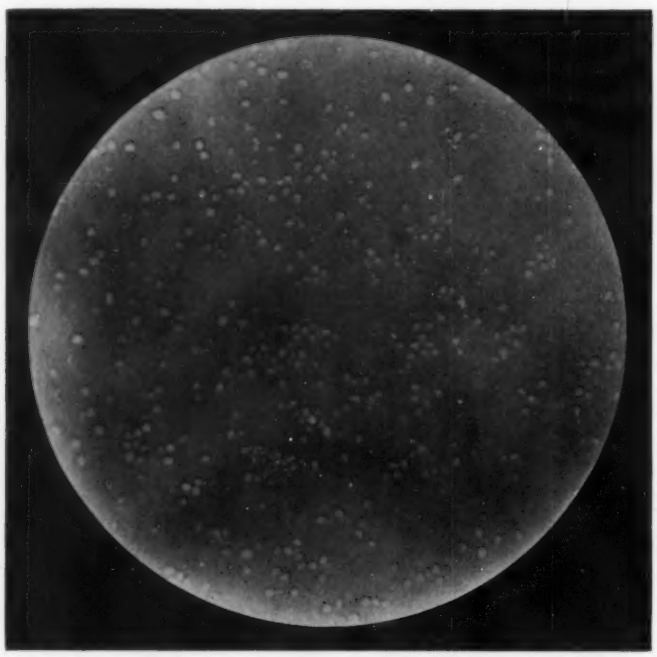

FIG. 6. Microphotograph of unexposed *Saccharomyces cerevisiae* (apl. cond. 1.4, obj. apochr. 20 ×, magn. 250, orange + green filter, Agfa Superpan, tungsten arc).

which are easily obtainable in pure culture, and where the individual cells are large enough not to necessitate the use of microscopic objectives of extremely short focal length, causing inaccuracies in the counts. The chief condition for experiments of surveying character seemed to be a sensitivity of the organism to exposure to low temperatures such that a sufficient number of individuals survive under normal (*i.e.*, not too

adverse) freezing conditions, and the non-occurrence of *CD* within the time of the experiment.

The yeast was obtained as commercial bread yeast and suspended in Ringer's solution, where it remains stable for a number of hours. For each set of experiments new yeast was used; this procedure was chosen as it proved to give more uniform results than those obtained with the same yeast taken

FIG. 7. Microphotograph of *Saccharomyces cerevisiae* once exposed to − 185° C. under vitrifying conditions (apl. cond. 1.4, obj. apochr. 20 ×, magn. 250 ×, yellow filter, Agfa Superpan, tungsten arc).

from liquid nutrient media. Another advantage is that the indicator staining method showed an equal resistance of all live cells to the stain, whereas cells taken from a nutrient culture show a varying percentage of individuals susceptible to the stain.

A general advantage of the use of such cells consists in the applicability to them of the technique of producing mono-

cellular layers on gelatine films (II*a*). This state is very easily checked in the ultra-microscope, where the superposition of two cells can be seen if present. Fig. 5 presents an ultra-microscopic view of such a monocellular layer of Saccharomyces cerevisiae.

For counting purposes, however, direct illumination has to be used since the ultra-microscope does not permit visual distinction between stained and unstained yeast cells.

FIG. 8. Microphotograph of *Saccharomyces cerevisiae* once exposed to − 185° C. (apl. cond. 1.4, obj. apochr. 20 ×, magn. 250 ×, orange filter, Agfa Superpan, tungsten arc).

III. EXPERIMENTAL RESULTS

(*a*) *Death Rate under Conditions Especially Favorable to Crystallization*

The yeast was suspended in Ringer's solution in concentrations of approximately 10^9 cells per cm^3. and cooled to the temperature of liquid air at comparatively low cooling rates.

The preparation was left at this temperature for a few minutes and then warmed to room temperature. The death rates thus obtained varied considerably for different experiments, as the method used did not permit of the exact reproduction of the cooling rates. The average values obtained are given in Table 1.

TABLE 1

Approx. cooling rate (deg. sec^{-1})	20	6	3	1
Approx. death rate (δ)	0.29	0.49	0.70	0.75

There are indications that with further decrease of the cooling rate a death rate of 100 per cent ($\delta = 1$) can be reached.

(*b*) *Death Rate under Conditions Especially Favorable to Vitrification*

The next step consisted in reversing the physical conditions of cooling by applying the principles set forth in Chap. II*c*.

The quantitative results presented in Table 2 are obtained with the loop-method (Chap. II*b*) for T_0 between $-150°$ C. and $-185°$ C. on yeast suspensions identical in nature, age, etc., with the suspensions used previously.

TABLE 2

($v \sim 10^3$ to 10^4 deg. sec^{-1})

t	N	D	δ	δ'
0	639	7	$0.011 = \delta_0$	
Circ. 2′	2232	194	0.087	0.076
Circ. 2′	1071	141	0.131	0.120
Total	*3303*		Average	0.098

As the submersion was done by hand the rate of cooling ($v = dt/dt$) is not expected to be the same in different experiments, for large values of v are required in view of the magni-

tude of V for aqueous solutions. In the subsequent tables the following symbols are used:

t = Time of exposure;
N = Total number of cells present in the photograph;
D = Number of stained cells;
$\delta = \frac{D}{N}$ = Apparent death rate;
δ_0 = Death rate already existing in the preparation before exposure to low temperature;
$\delta' = \delta - \delta_0$ = Actual death rate by exposure;
v = Rate of cooling.

Figs. 6 and 7 represent photomicrographs of preparations before and after exposure under these conditions.

In accordance with condition 3 (Chap. I*c*) the effect of colloidal admixtures upon δ was tested with the following results (C_g gelatine concentration).

TABLE 3

($v \sim 10^3$ to 10^4 deg. sec^{-1})

C_g	t	N	D	δ	δ'
0	0	822	57	0.069 = δ_0	—
1/2500	2′	501	74	0.147	0.078
1/250	2′	447	67	0.149	0.080
1/50	2′	390	50	0.128	0.059

Although the material so far collected is not sufficient in volume for a final quantitative statement it is evident that:

1. exposure to low temperatures under vitrifying conditions increases the percentage of surviving cells from 25 per cent to 90 per cent.

2. addition of gelatine decreases δ still further.*

This appears to prove the previous postulate that *PD and crystallization of the cell are defined functions of each other, and*

* The results in table 2 are unfavorable to δ because of the high value of δ_0 which indicates a low vitality of the particular preparation used.

that by supressing the crystallization the death rate can be controlled.

The definite proof depends upon a quantitative determination of $\delta = f(v)_{T_0}$ for which the primitive methods employed so far are insufficient. Results with improved apparatus will be published soon.

(·) *Death Rate under Conditions Favoring the Coexistence of the Vitreous and the Crystalline State*

In order to determine the influence of the temperature, duration of exposure etc., the cooling conditions for the following experiments were chosen so that δ ranged reproducibly between 0.25 and 0.50 in order to dispense with the necessity of counting very large numbers of cells for obtaining sufficient accuracy.

1. Death rate as function of duration of exposure to low temperatures ($\delta = f(t)_T$).

The procedure of cooling chosen to meet the above condition for the study of the variation of δ with t, was as follows:

A given quantity (5 c.c.) of a standard suspension of yeast in Ringer's Solution was put into a test tube and submerged in liquid air, in order that a relatively slow cooling rate might obtain. A number of tubes were prepared in this manner and exposed for different lengths of time to − 185° C., the shortest time being 5′ (the minimum time for the complete cooling and thawing process) the longest 6000′. Precautions were taken that the warming speed be the same for all samples. The slides were then prepared according to the above method and the results, condensed in Table 4, were obtained.

In Fig. 9 the results are plotted vs. the logarithm of the time of exposure, where the diameters of the circles (●) represent the estimated accuracy of the counts.

The results obtained show a good consistency of the values and indicate that, in agreement with postulate of condition 4 in I*c* *the duration of exposure does not affect* δ, even if T is

TABLE 4

t	lgt	N	D	δ	
0		1312	16	0.012 = δ_0	
5′	0.70	1309	405	0.309	0.318
5′	"	1017	334	0.328	
17′	1.23	1252	391	0.312	0.324
17′	"	1191	402	0.337	
60′	1.78	2145	655	0.305	
120′	2.08	1489	431	0.289	
210′	2.32	1216	348	0.285	
1080′	3.03	858	291	0.339	
600′0	3.78	1976	641	0.324	
Total		*12453*	Average	0.312	

varied by a factor > 1000, *if the temperature is far below the critical temperature interval.*

The reaction of an older yeast suspension (48 h) to − 185° C. under the same conditions shows a higher δ (0.48), indicated in Fig. 9 as ●.

2. Death rate as function of temperature ($\delta = f(T)_t$).

In order to test the conclusion made previously that below the "critical" temperature range the temperature should not have any influence upon the death rate, a preparation identical with those described in (III.*c*.1) was exposed to the boiling temperature of hydrogen (− 252° C.). The experiment was performed as follows: 1 c.c. of the suspension was sealed in a glass tube and submerged first in liquid air for the purpose of precooling. It was then transferred quickly into a container of liquid hydrogen and left there for approximately four hours. It was then transferred again into a liquid air bath, where it remained 24 hours. Then it was warmed to room temperature

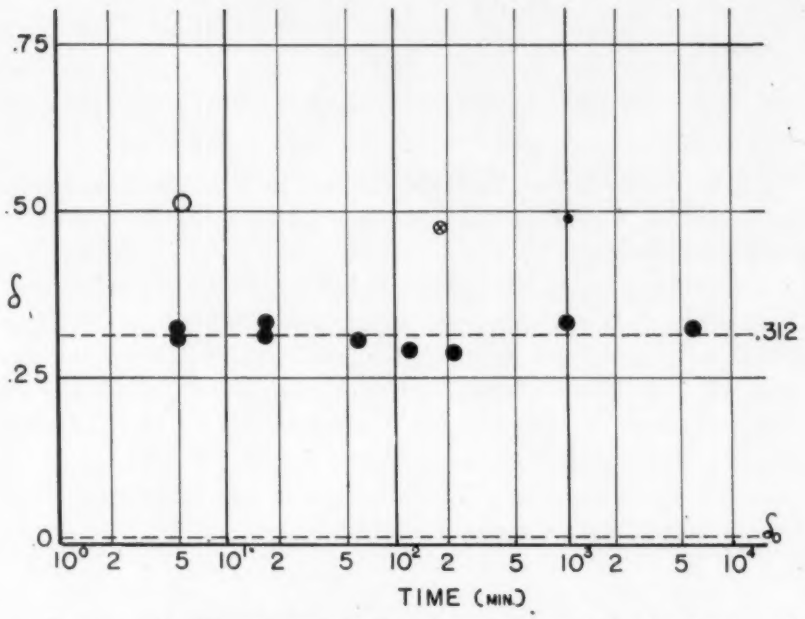

FIG. 9. $\delta(t)$—Diagram representing the invariance of the death rate with time of exposure, the latter being represented on a logarithmic scale. δ_0 = death rate for the unexposed suspension; ● for fresh suspension at − 185° C.; • old suspension at − 185° C., ○ for fresh suspension exposed to − 50° C. ⊕ old susp. at − 252° C.

at the normal rate, after which δ was measured in the usual manner.

The suspension was of 48-hour age, characterized at liquid air temperature by $\delta = 0.48$ (see III.*c*.1). The value obtained in the liquid air—hydrogen exposure (⊕) proved to be $\delta = 0.46$, which is, within the experimental error, identical with the liquid air value. This indicates *the invariance of* δ *with T below the critical temperature range.*

Although sufficient data are not yet available for a reliable indication, it may be mentioned that similar experiments have been performed at − 50° C., that is, within the critical temperature range (marked by ○ in Fig. 9) by means of pentane cooled in a solution of CO_2 in acetone. The value of δ, under otherwise similar conditions, proves to be about 0.5 for an exposure of several minutes for a freshly made suspension,

i.e., 18 per cent higher than the average $\delta = 0.31$ for a fresh suspension at $-185°$ C.

c. Death rate as function of the number of repeated exposures to low temperature ($\delta_n = f(n)_{t,\ T_0}$.

A different type of experiment can be devised in order to gain insight regarding the nature of *PD* in organic matter from another angle.

If the same sample is *repeatedly exposed* to low temperatures under identical cooling and warming conditions (v, T_0), the following simple mathematical consideration should be applicable. If N is the total number of cells, D_n the number stained (dead), L_n the number unstained (alive), both after n the nth exposure, then: $\delta = D_1/N$; $\lambda = L_1/N$; $\delta = (1 - \lambda)$ after the first exposure. The total death rate (determined by count) after the nth exposure is therefore:

$$\delta_n = f(n)_{u,\ T_0} = \sum_1^n \delta \cdot \lambda^{n-1} = \frac{\delta(\lambda^n - 1)}{\lambda - 1} = 1 - \lambda^n.$$

The experiments were conducted under similar conditions to those described in III.*c*.1, *i.e.*, for medium values of $\delta(= \delta_1)$. Table 5 represents the results together with calculated values of δ_n for comparison.

TABLE 5

n	N	D_n	δ_n	$\delta_n = (1 - \lambda^n)\delta_1$	$\delta_n = (1 - \lambda^n)\delta_2$
0	1312	16	0.012	—	—
1	1200	401	0.333	0.333	0.423
2	1359	902	0.667	0.552	0.667
3	1370	1070	0.781	0.700	0.808
4	840	669	0.796	0.799	0.889
5	—	—	—	0.865	0.936
Total	6081				

Figure 10 illustrates the difference between the experimental and the theoretical function, which later has been calculated up to $n = 5$ for $\delta = \delta_{1\ \mathrm{exp.}}$ and for

$$\delta = 1 - (1 - \delta_2)^{1/2}.$$

The observations show a systematic deviation from the theoretical functions beyond the experimental error, as $\delta_{n\ exp.}$ (n) increases faster for $n = 2, 3$ and again reaches numerical equality for $n = 4$ with the expectation of decreasing below

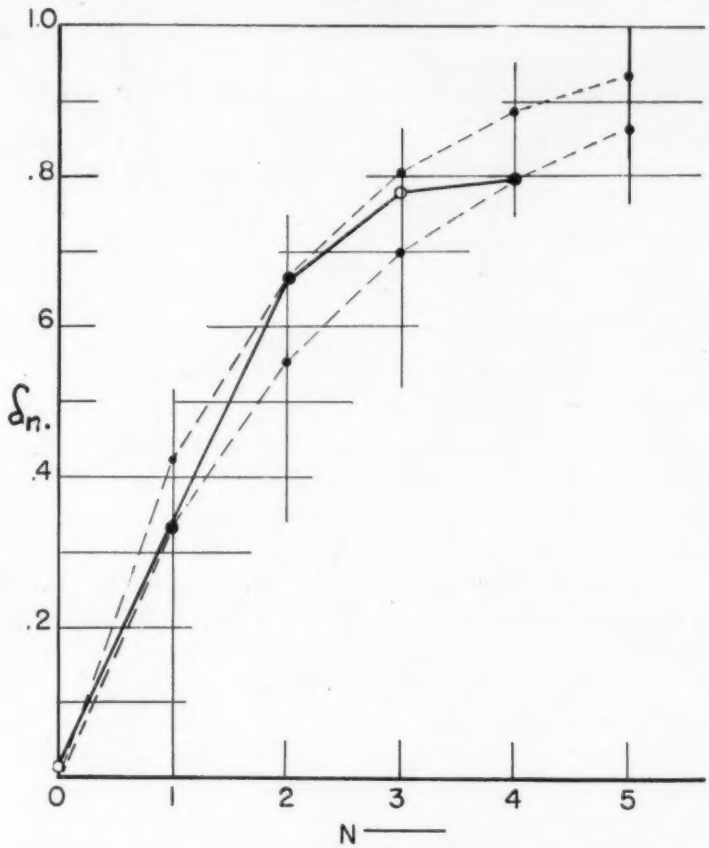

FIG. 10. $\delta_n(n)$—Diagram representing the integral death rate after repeated exposure to − 185° C. under normal cooling conditions. The full drawn lines represent the observations, the dashed lines the prediction for δ = const., the lower for $\delta = \delta_1$, the upper for $\delta = 1 - (1 - \delta_2)^{1/2}$.

the theoretical values for $n = 5$. In other words, the behaviour of the function indicates that δ *does not stay constant with repeated exposures*, for δ_2 and δ_3 are larger than δ_1 and δ_4. Although the number of counts and experiments are, up to

now, not sufficient for a definite statement, the possible causes for this type of deviation will be briefly discussed.

1. The first exposure weakens a certain fraction of the cells against *PD* but they are not destroyed. These cells cannot resist a second or third exposure and thus contribute largely to δ_2 and δ_3. After all such cells have been destroyed, δ_4 is again normal (the close numerical coincidence for δ_4 in Table 5, is of course to be considered accidental). It is also to be expected that for δ_5 the values would be even smaller than predicted because the remaining cells represent a selection of those with the most resistant qualities.

With this assumption it appears nevertheless surprising that the first exposure does not show the largest death rate.

2. A large fraction of cells killed in previous freezings is physically annihilated by crystallization (bursting the cell membrane) during later exposures to alternatingly low and high temperatures. Thus with each new freezing, the number of cells killed during previous exposures is gradually decreased, as these burst cells become invisible and drop out of the count.

In order to make certain, it is necessary to modify the present method of counting to include a measurement of the concentration of the suspension. Experiments of this type have been performed with the use of a hemacytometer modified for this purpose. The results so far obtained indicate qualitatively that such a phenomenon actually takes place.* Further results along this line will be published soon.

This effect, however, cannot explain the increase of $\delta_{2,3}$ over δ_1, for which only the physiological cause previously discussed appears to account.

The possibility of "weakening" cells by exposure gains probability in view of the fact that in these last experiments small cooling velocities were used, possibly allowing time for destruction by *CD*. It is to be expected that the $\delta_n(n)$ function will change considerably for forced cooling conditions. Work in this direction is continued.

* Recent experiments in which the total population was determined with the counting chamber after each consecutive n seem to indicate that the number of cells stays more or less constant and that, in any case, the decrease measured cannot be made to account for this effect.

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NEUTRAL PARTICLES IN PHYSICS

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(*Read April 21, 1938*)

ABSTRACT

During the early days of science, the elementary particles which scientists and philosophers alike saw fit to postulate were always imagined as chargeless. With the remarkable growth of the understanding of electricity during the nineteenth century, and with the invention of instruments for detecting small charged particles during the twentieth, it became the custom to suppose that the fundamental particles of matter all bear charges and that the forces exhibited in Nature are all electrical (exception being made for gravitation). A noted and serious objection to this view was temporarily met by the adoption of quantum mechanics. Since 1930 a reversal of trend has set in, heralded by the discovery of the neutron as a subatomic chargeless particle capable of independent existence; and at present there is a strong tendency to develop the view that neutral as well as charged particles of subatomic size, and non-electrical as well as electrical forces, exist together in Nature.

I HOPE that it may entertain you to hear some general remarks on the status of fundamental neutral particles in our present idea of the world. Right at the outset I flounder into difficulties with my phrasing, for there is a certain amount of presumption and risk in referring to any known particle by the word "fundamental." Of course there is no objection to applying that word to, let me say, the atoms of Lucretius. Lucretius never pretended to have observed the atoms which he talked about; he merely imagined them, and so he could define them in such a way that they couldn't help being fundamental by definition. But when a physicist refers to a so-called fundamental particle, he is referring to something which can be and has been observed. Therefore he is always exposing himself to the risk that somebody else, or possibly he himself, may some day break it up. It is entirely unsafe to affirm that any of our particles is really ultimate. I think that the best way to avoid this linguistic hazard is, to share

out the term "fundamental" among several kinds of particles, of which we already know that some are composite and are willing to concede that all the rest may be. Here is my list of today, with symbols at the right to indicate the charges:

1. Atom	0
2. Negative electron	−
3. Nucleus	+
4. Proton	+
5. Neutron	0
6. Positive electron	+
7. Photon (corpuscle of light)	0

The atom which heads the list is not, of course, the imaginary ultimate particle of Lucretius. It is, or rather they are, the chemical atoms—the atoms of the chemical elements. Of these we all know that they are composite. An atom is in fact regarded as a sort of miniature solar system in which a nucleus plays the role of the sun and a host of negative electrons play the roles of the planets; only, the forces which hold the system together are electrical and not gravitational. This is the first of the points—not new points, of course, but very well-known ones—which I want to stress. Our atom-model is electrical entirely; it has none but electrical forces inside it; it is a product and an epitome of the electrical theory of matter. For the best part of forty years, physicists have been systematically denying the right of any but the electrical forces to operate in the heart of matter. Possibly that sounds like an over-statement, in view of the fact that after all no physicist has yet denied gravitation, and the further fact that all our literature is full of allusions to chemical affinities, chemical forces, valence forces, capillarity forces, and I don't know what else. However my statement cannot be attacked on these grounds; for as to gravitation, it has always been considered *sui generis* and recognized as being far too weak to play a great role in the interior of the atom; and as to chemical affinity and capillarity and all the rest, we have long been taking it for granted that all of these forces must be electrical. Indeed I am afraid that physicists have rather got into the habit of

regarding as a crank anybody who believes in any forces not electrical! So great has been the prestige of the electrical theory of Nature.

An odd thing is, that the electrical theory of matter got itself established with this great prestige despite the broadly-known fact that it is demonstrably impossible. There is a classical theorem to the effect that a system of electrical charges cannot be in stable equilibrium if the charges are at rest, and cannot be in any sort of a durable state at all if the charges are in motion. This apparently defeats at the outset the ambition of constructing a thoroughly electrical world. Now I am not going to retell you the many-more-than-thricetold story of how Bohr met this difficulty. Bohr snatched victory from defeat by deliberately ignoring that theorem. You recall how Polonius, after listening to Hamlet for a while, decided that there was method in his madness. Well, Bohr's idea *was* a crazy one, as plenty of people in the early days did not hesitate to say; but there proved to be so much method in that madness, that eventually the method came to be worked up into a whole new system of mechanics and the madness ceased to be regarded as madness. Now I can amend my statement of a minute ago about the electrical theory of matter. It is the theory that all neutral bodies are aggregates of charged particles and that all the forces in Nature are demonstrations of the electrical forces between these particles, operating according to the new or quantum system of mechanics. This is the theory that attained such prestige that it drove the very idea of neutral elementary particles completely out of mind. How completely, I will now illustrate to you.

We turn from the atom to the nucleus thereof. Looking over any tabulation of the several hundred types of atom-nucleus with which chemists and physicists are already acquainted, we see many places in which two of the types are identical with each other in respect to charge (they are said to be different isotopes of one and the same element) but differ from each other by about one unit of mass. "One

unit of mass" signifies a mass about equal to one-sixteenth of the mass of the oxygen atom. (It would be necessary to refine these statements with considerable care if in this paper I were concerned with precise mass-values, but for my present purposes these phrasings are not too rough.) Instances of this kind are furnished by Li^6 and Li^7, Li being the symbol of a chemical element, while the superscripts 6 and 7 stand for the masses of two of the nucleus-types belonging to this element; by He^3 and He^4, the two known isotopes of helium; by H^1 and H^2, the famous "ordinary hydrogen" and "heavy hydrogen," of which the nuclei are known by the names of "proton" and "deuteron" respectively. It is evident that the simplest way to visualize the Li^7 nucleus is, as a Li^6 nucleus with one extra neutral particle of mass one; that the simplest way to visualize the He^4 nucleus is, as a He^3 nucleus with one extra neutral particle of mass one; that the simplest way to visualize the H^2 nucleus is, as a proton with an extra neutral particle of mass one. Yet nearly everyone preferred, until six years ago, to visualize the Li^7 nucleus as a Li^6 nucleus with *two* extra charged particles (proton and negative electron) having equal and opposite charges and a total mass amounting to one mass-unit; and had the heavy isotope of hydrogen been discovered earlier than it was, there would undoubtedly have been a period of visualizing it as consisting of three particles all of them charged and two of them compensating one another's charges (two protons and one negative electron) rather than as consisting of two particles one of them charged and the other not. The simpler model was deliberately rejected in favor of the more complex. This state of mind continued until at last the experts in the art of transmutation managed to operate on the nuclei in such a way as to detach these neutral particles and set them free to wander forth into space and prove their existence in independence.

There are now upwards of a hundred known ways of detaching this hypothetical particle, which on being set free ceases to be hypothetical. Shakespeare said that the poet's

pen could give a local habitation and a name to the forms of things unknown, but here the unknown corpuscle acquired a name only on being thrown out of its local habitation. The name, of course, is "neutron." The simplest and one of the handiest ways of releasing a neutron consists in projecting one deuteron violently against another, with the aid of a high-voltage machine: it then happens—not always, but sometimes—that one of the deuterons takes the proton away from the other, leaving the neutron to go free. There are scores of other ways, some of which were sooner discovered. When the release of the neutron was definitely confirmed, the world of physics relinquished the nucleus-models till then accepted, and began to imagine the atom-nuclei as systems of protons and neutrons only.

But how is the neutron observed when it has been freed? Here comes another important point. Since 1932 our minds are no longer constrained to conceive electrical particles only; but our apparatus is still traditionalist. The fundamental particles can manifest themselves to it directly, if and only if they have charges. If they do not have charges they must induce charged particles to manifest them vicariously. "Induce" is after all a pretty weak word for the process in question, which is more like assault than it is like persuasion. The liberated neutron, rushing at high speed through space, happens to strike a forcible blow against an atom-nucleus, preferably a proton. This struck particle recoils from the impact, and being charged is able to manifest itself to us through our apparatus. Anyone who has travelled in France has probably noticed that the verb *inviter* has a somewhat different flavor from our supposedly-equivalent *invite*; for one reads (for instance) that a crowd assembled in a place where it was not desired, and the policeman came and *invita* its members to go elsewhere. It is in this sense that the free neutron, happening to be advancing straight toward a nucleus, invites the latter to go elsewhere. I do not play billiards, which is certainly a defect in a person who must refer to billiard-balls as often as a physicist does; but I have

at least looked upon billiard-tables, and I have noticed that sometimes there are balls of two different colors. Suppose that on the table there were, not white balls and red balls, but white balls and invisible balls. Conceivably one might infer the presence of the invisible balls, by noticing that every now and then one of the white balls started off at a great speed without any visible cause. Of course somebody might conclude the existence of invisible players instead of invisible balls, but that would be an animistic way to look at the situation instead of a scientific way. In Nature—that is to say, in our apparatus—the neutrons correspond to the invisible balls, the nuclei to the white ones. We know of the free neutrons, because every now and then one of the nuclei will start off in precipitate flight. There is another way of detecting the neutrons, depending on the fact that when they impinge on nuclei of certain kinds they enter these and cause them to explode: the flying fragments of these explosions are observable because and only because they are charged, and so we come back to the same techniques.

Now I must say that the neutron is not the first-to-be-discovered of *all* neutral particles, but only the first-to-be-discovered of those which have the quality of matter. The earliest-to-be-discovered of all the fundamental neutral particles are the corpuscles of light, the photons. The individual photon seems to be just as powerless as the individual neutron to manifest itself to us directly. It has to invite a charged particle for the purpose, and to strike it a blow—or, to use a gentler and more technical phrase, it has to confer some of its own energy on the charged particle. Whereas the invitations of neutrons are directed to nuclei, those of the corpuscles of light are addressed to electrons. There are interactions of various kinds between electrons and photons, and for one at least among them it is permissible to use that image of the colliding billiard-balls. Let me now add that when a physicist speaks of billiard-balls, he is using a metaphor for what in a professional paper he would call "strong short-range forces." These forces are non-electrical,

or at any rate if we were to call them electrical we should be distorting the word out of sense.

But how about the possibility that these neutral particles may be, each of them, a pair of charged corpuscles locked so tightly into each other's arms that to the outside world their charges seem totally annulled while yet remaining distinct?

At first glance this appears to offer a chance of saving or reviving the electrical theory of matter. Not so much can be expected of it, since now and henceforward the short-range or billiard-ball forces must be implanted in every conception of either charged or chargeless particles. Little or nothing would be gained by trying in imagination to link chargelessness with charge, were it not for some quite remarkable phenomena which more than justify the attempt. The most striking of these are the conversion of individual photons into pairs of free electrons, one member of the pair being negative and the other positive; and the conversion of electron-pairs into photons. Though known for barely four years as yet, both of these phenomena have been detected many times over in many ways; and while the experiments still want the last refinements of accurate measurement, there is no reasonable doubt remaining. Now, no one maintains that a corpuscle of light is nothing but a pair of electrons close together. There are too many objections to such a simple idea; anyone can think of several, I will mention only that light in vacuo is constrained to a single definite immutable speed and electrons suffer from no such limitation. The most that here can be asserted is, that equal and opposite charges come out of apparent chargelessness, and equal and opposite charges return into apparent chargelessness, and other remarkable changes occur at the same time.

Do equal and opposite charges ever come out of the neutron, and do equal and opposite charges ever meet and form a neutron in disappearing? Nothing of the kind has yet been observed, and there is in fact a strong reason for disbelieving in the quite obvious idea that a neutron may be a proton and a negative electron tightly clasped together.

Nevertheless there is the disconcerting fact that electrons are observed to spring out of nuclei—out of more than two hundred different kinds of "radioactive" nuclei. I call it disconcerting, because I have been trying to make you picture every nucleus as a composite of nothing but protons and neutrons, and here I suddenly burst out with the admission that these alleged composites of protons and neutrons are perfectly capable of projecting free electrons into the outside world! I must further confess that this very affair of the ejection of negative electrons from nuclei was precisely one of the reasons that made people prefer the more complicated nucleus-model which I invited you to reject, over the simpler one which I recommended you to accept. Admittedly I was not being fair to the complicated model. However the chances are that I was not misleading you. The people of the days before 1932 who believed in the proton-and-negative-electron model of the nucleus did not know nearly so much about the variety of Nature as they were soon to learn. They did not know that neutrons can be detached from nuclei, or that some radioactive nuclei emit electrons which are positive instead of negative. So, if one insists on putting into the nuclear zoo at least one specimen of every kind of animal which has been observed to come out—if one insists upon that, then one has to make a model containing at the very least four kinds of fundamental particles: neutrons, protons, electrons of both signs. This is so complicated a prospect, that of late the theorists have been trying out the alternative idea: the idea that the electrons which are fired out of radioactive nuclei are created just before they depart. It is conjectured that the neutron in the nucleus may possess the power of converting itself into an electron of either sign, and a residue which would either be a proton, or a particle equal in mass but opposite in sign of charge to the proton.

I do not guarantee these ideas—not that my guarantee, or the lack of it, would make the slightest difference to their proponents—and I certainly do not present them to you on the same footing as the conversion of photons into electron-

pairs. This is a fact of experience, and an addition to knowledge; those are illustrations of the trend of thought. All of them anyhow illustrate my point that the neutral particles and the non-electrical forces have come back into physics after their exile of nigh on forty years, and that we have both the fact and the speculation of electric charge coming into being out of neutral mother-substance and returning into it again. Of course this may be a very transitory stage in modern physics. However at least for the present we may feel assured that even the wisest of theorists would not dare to debar us from contriving a cosmos in which neutral particles exist as well as charged ones, non-electrical forces as well as electrical, and perhaps even charge is secondary and a neutral mother-substance primary.

THE MAP OF LATIN AMERICA BY TREATY

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(Communicated by Waldo G. Leland)

(*Read, April 22, 1938*)

ABSTRACT

The historical materials for the study of Latin American boundaries are widely scattered, in publications of many countries, and in several languages. They are difficult to find, and to use when found. Overlapping claims involving three or more countries have made the boundary disputes and negotiations surprisingly complex. For these reasons, and because of the unusual importance of boundary questions in Latin American history, scholars, writers and statesmen are seriously handicapped by the lack of a compilation analogous to Hertslet's "Map of Europe by Treaty."

To illustrate the problems involved, the history of overlapping claims of Brazil, Ecuador, Colombia and Peru is reviewed. Problems derived from inadequate geographical knowledge are illustrated from old maps of the Honduras-Nicaragua area.

A comprehensive study of Latin American boundaries seems to require two publications:

(1) "The Map of Latin America by Treaty," containing the texts of papal bulls, treaties of the colonial and republican periods, arbitral awards, etc., arranged chronologically; each in the original language with English translation; each accompanied by explanatory notes, many by maps.

(2) "A History of Latin American Boundaries," containing the history of the establishment of each of the 38 boundaries from the Rio Grande southward.

As the first step, a pamphlet should be prepared containing:

I. A chronological list of documents contemplated in "The Map of Latin America by Treaty."
II. A classified list of the same documents arranged by country and by individual boundary.
III. A bibliography of treaty collections, etc.

The library resources of Washington are probably unexcelled for such a study. The map of Hispanic America prepared by the American Geographical Society of New York affords an excellent cartographic base.

THE historical materials in which the successive stages of the development of international boundaries of Latin America are recorded are widely scattered in publications of the twenty

Latin American republics, Spain, Portugal, Great Britain, France, the Netherlands, and the United States. They are difficult to find, and perhaps equally difficult to use when brought together. Overlapping claims of three or four countries within a single area account for interrelationships between bilateral boundary disputes and negotiations which are surprisingly numerous and complex. In the words of an eminent authority:

> . . . It may be said that a very considerable part of the diplomatic history of Latin-America reduces itself to an account of the struggle over boundaries. For this reason, and because of their great political and economic significance, these contests occupy a place of capital importance in that history.[1]

It may therefore be appreciated that scholars, writers and statesmen are seriously handicapped by the lack of any compilation relating to boundaries in the Americas which is analogous to Hertslet's "Map of Europe by Treaty" or Hertslet's "Map of Africa by Treaty."

The Latin American republics, throughout more than a century of independence, have undertaken generally to establish their boundaries on the principle of *uti possidetis*, that is, on the basis of the extent, at the time they established their independence, of the territory possessed by the several viceroyalties, captaincies-general and other entities from which they evolved as independent sovereign states. The natural tendency has been for each country to apply this principle in a manner that would give it a maximum domain. A long series of boundary disputes, of which several are even now unsolved, may be traced to vaguely defined limits of administrative and ecclesiastical divisions in poorly mapped areas, and to numerous transfers of territory from one colonial jurisdiction to another.

Some of the problems involved in considering the boundaries in Latin America may be illustrated in a region in which four of the ten South American republics meet, namely

[1] John Bassett Moore: Memorandum on uti possidetis, Costa Rica-Panama arbitration (1913), pp. 14–15.

Brazil, Ecuador, Colombia and Peru. In an area of widely overlapping claims, each of four such countries may have a dispute with each of the three other neighbors; the six pairs of countries thus afford six boundary disputes. But with four countries situated as these are (no one of them being surrounded, as is Bolivia or Paraguay), there could not possibly be six boundaries. Once the disputes were disposed of, one of the six must have been pinched out, leaving five actual frontiers between the four countries.

As the boundaries have become established the possibility of a boundary between Brazil and Ecuador has been eliminated. The other five have become actualities, including the undefined Ecuador-Peru frontier.

At the risk of over-simplification, these six boundary controversies will be sketched, illustrated by lantern slides.

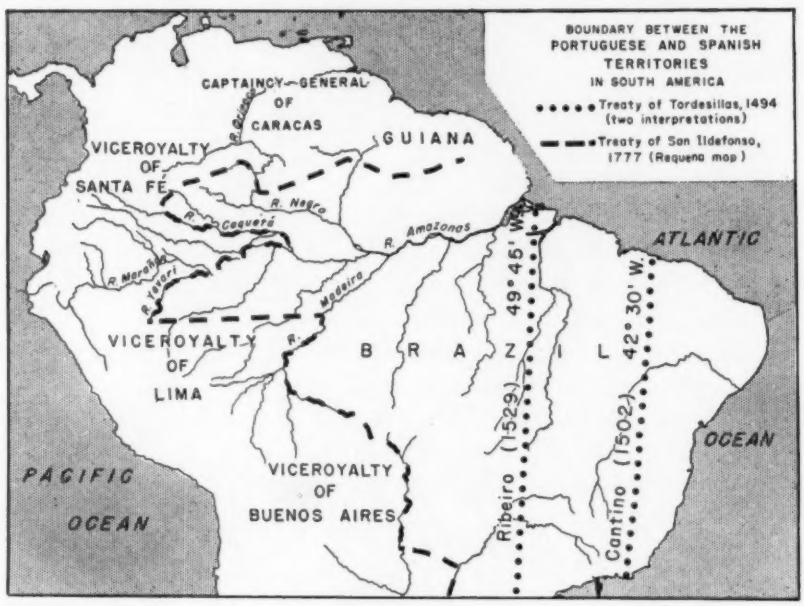

FIG. 1.

The boundaries of Brazil, as you are aware, are based in general upon the line separating the territories of the Crowns of Portugal and Spain at the time when it became independent,

in 1821. The line of the treaty of Tordesillas, 1494, a meridian 370 geographical leagues west of the Cape Verde Islands could not be located accurately at that time, because of uncertainties regarding the initial point in the Cape Verde Islands, the extent of a geographical league in relation to the circumference of the earth, and the impossibility of determining longitude reliably at sea until well more than a century later. The Tordesillas line gave Portugal a foothold in Brazil. Two of the early attempts to lay down the Tordesillas line [1] are shown on Fig. 1. The longitude difference between the two meridians is greater than that between Philadelphia and Cleveland.

The treaty of Madrid, 1750, and the treaty of San Ildefonso, 1777, made at least a preliminary attempt to delimit a Portuguese-Spanish boundary. The San Ildefonso line, as represented on the map made by Francisco Requena,[2] 1796, is shown on Fig. 1. The viceroyalties of Santa Fé, Lima and Buenos Aires, and the captaincy-general of Caracas, as shown on the Requena map without boundaries, are likewise indicated.

On the maps that follow it will be shown that all four countries have claimed territory between the Caquetá or Yapurá and the Amazon; and that at least three countries have claimed territory north of the Caquetá. To the triangular controversies between Venezuela, Brazil and Colombia, and likewise between Bolivia, Brazil and Peru in the Acre region, we shall make only passing reference.

As a background for a brief review of the disputes between the four republics, Fig. 2 shows the boundaries as they are today, in part by means of distinctive shadings. All of them within the area of the map are now established except that between Ecuador and Peru.

[1] Henry Harrisse: The Diplomatic History of America. Its First Chapter, 1452–1493–1494. London, 1897. Map facing title-page.

[2] "Mapa geográfico de la mayor parte de la América Meridional que contiene los paises por donde debe trazarse la línea divisoria que divida los dominios de España y Portugal, construida en virtud de Real Órden por el Teniente General D^{n}. Francisco Requena en el año de 1796. Printed by F. Bourquin, 31 So. Sixth St., Philadelphia, Pa."

In its various boundary disputes, Colombia has in general based its territorial claims upon the extent of the viceroyalty of Santa Fé or New Granada, according to royal cédulas of 1717 and 1739, minus the audiencia of Quito. Ecuador has claimed the territory of the audiencia of Quito, created in 1563, belonging alternately to the viceroyalties of New Granada and Lima. Peru has in general claimed the territory

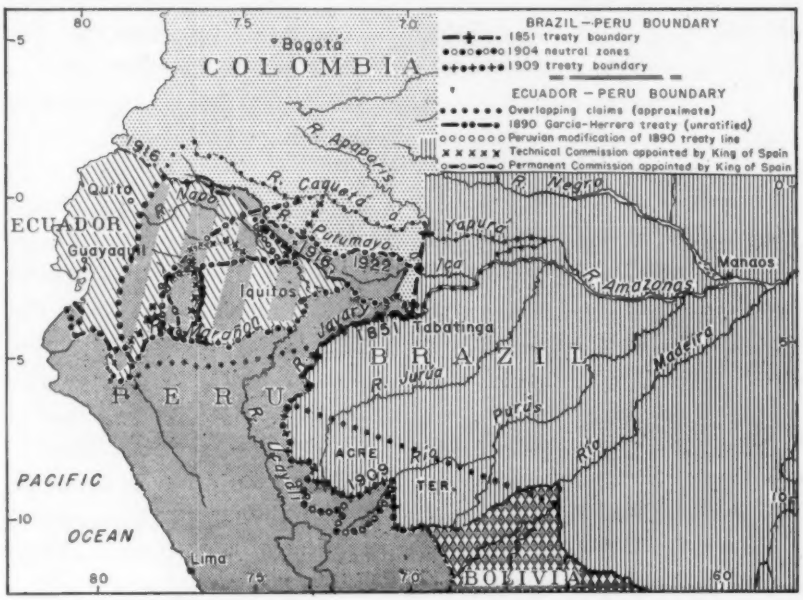

FIG. 2.

of the viceroyalty of Lima or Peru, citing early cédulas and also one of 1802 which enlarged its borders shortly before independence. Brazil, as has already been indicated, has claimed territory based chiefly on the extent of Portuguese possession in 1821, but it has sometimes maintained that subsequent effective occupation of portions of the upper Amazon basin was consistent with the *uti possidetis* principle.

BRAZIL-PERU BOUNDARY

Brazil and Peru by treaty in 1851 defined their boundary as constituting the straight line from the mouth of the

Apaporis River in the Caquetá, to Tabatinga on the Amazon (we shall necessarily hear more of this Apaporis-Tabatinga line), and from Tabatinga southward, the Javary River to its source (Fig. 2). Colombia immediately protested, denying the right of both Brazil and Peru to the territory traversed by the Apaporis-Tabatinga line. A minor modification was made in 1874, where the line cuts the Putumayo. The Bolivia-Brazil dispute in the Acre area having been settled by treaty in 1903, Peru and Brazil in 1904 established neutral territories in the upper Juruá and Purús river basins. In 1909 they extended their boundary from the source of the Javary to the point where their respective frontiers with Bolivia begin.

Ecuador-Peru Boundary

Ecuador and Peru made several attempts to establish their common boundary, beginning in 1832. However, no treaty between them has ever gone into effect which defined any portion of their frontier. The Garcia-Herrera Treaty of 1890 defined the entire boundary, from the Apaporis-Tabatinga line to the Pacific Ocean. Ecuador ratified it; Peru ratified it with territorial modifications which Ecuador rejected. Colombia objected to the proposed settlement, and a tripartite convention was negotiated in 1894, which was never perfected by the exchange of ratifications.

Fig. 2 shows approximately the area in dispute, totaling 40,000 or 45,000 square miles—the area with alternate shadings characteristic of Ecuador and Peru, partially surrounded by a line of large heavy dots. A larger area formerly claimed by the two countries is indicated by the similar line of smaller dots. The maximum former claims of Ecuador and Peru in their dispute have included much that is now in Colombia, and part of Brazil. The area once claimed by Peru included Guayaquil. The Garcia-Herrera Treaty line, with the Peruvian modification, and the lines of the two Commissions appointed by the King of Spain, are shown running through the disputed area.

In the arbitration before the King of Spain, under the treaty of 1887, Ecuador claimed the ancient boundary between the viceroyalties of New Granada and Peru, based on the cédulas of 1717 and 1739, that is, at the time of the creation of New Granada. Peru claimed the boundary between the same viceroyalties based in part upon a royal cédula of 1802.

Under the terms of a protocol of 1924 delegations of the two countries have been negotiating in Washington since 1936, but thus far have not succeeded in reaching an agreement on the boundary.

Brazil-Ecuador Boundary

Brazil and Ecuador by treaty in 1904 agreed that if the Ecuador-Peru boundary dispute were settled in favor of Ecuador the Brazil-Ecuador boundary should be, wholly or in part, the Apaporis-Tabatinga line defined by Brazil's treaties of 1851 and 1874 with Peru. The treaty further declared that the two high contracting parties had no intention of prejudicing the rights that might be subsequently established by neighboring countries, referring specifically to the boundary question between Brazil and Colombia, and to the triangular questions between Ecuador, Colombia and Peru. Subsequent developments may be said to have pinched off Ecuador some distance to the west of the Apaporis-Tabatinga line, Colombia and Peru intervening and thus excluding the possibility of Ecuador and Brazil being contiguous states.

Colombia-Ecuador Boundary

Colombia and Ecuador made several attempts to establish their frontier, beginning with a treaty of peace and friendship in 1832. The treaty of 1916, however, is the only treaty that ever went into effect between Colombia and Ecuador that defines any portion of their common frontier (Fig. 3). The entire boundary was defined from the Pacific Ocean to the Marañon or Amazon River. A considerable portion of it has since been demarcated.

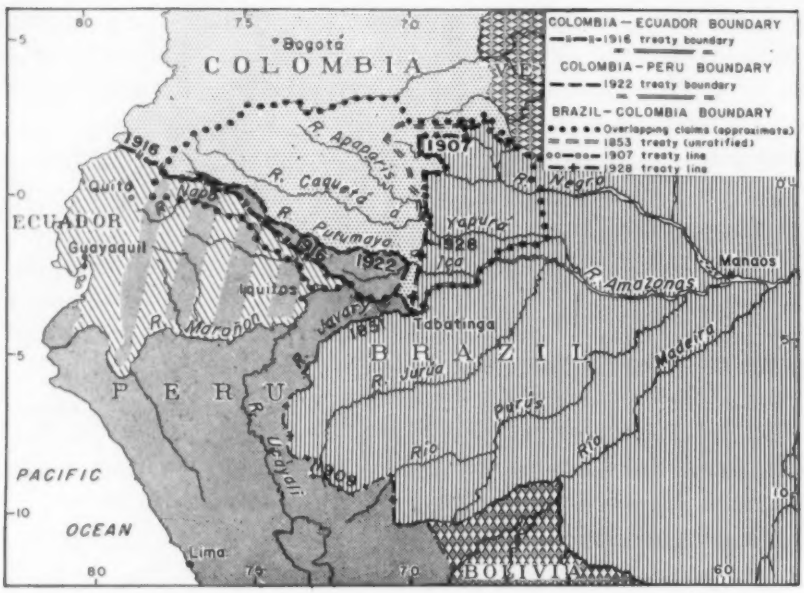

FIG. 3.

About two-thirds of the boundary has been eliminated as a frontier between Colombia and Ecuador, by the Colombian cession of a long strip of territory to Peru.

COLOMBIA-PERU BOUNDARY

The Colombia-Peru boundary was defined by treaty in 1922, ratifications being exchanged in 1928. It will be observed (Fig. 3) that much of the line lies to the north of the Colombia-Ecuador 1916 boundary. Colombia conceded to Peru, therefore, a long, narrow strip of territory which Ecuador had previously conceded to Colombia, lying south of the Putumayo River, and north of the Putumayo-Napo divide. Ecuador had agreed to the Putumayo as its boundary with Peru, by the Garcia-Herrera treaty, on the assumption that Ecuador rather than Colombia would be sovereign of the territory north of the Putumayo. By the 1922 treaty with Colombia, Peru now surrounds on three sides territory that is claimed by Ecuador.

By the same treaty Colombia acquired a corridor to the Amazon River immediately to the west of the Apaporis-Tabatinga line—the so-called Leticia quadrilateral. Colombia furthermore reserved its right to claim territory to the east of the Apaporis-Tabatinga line which Brazil had regarded as having been established as Brazilian territory by its treaties of 1851 and 1904 with Peru and Ecuador respectively. To this disconcerting provision Brazil objected. A triangular dispute arose between Brazil, Colombia and Peru which remained unsettled until the Gordian knot was cut by the protocol of Washington, signed March 4, 1925. The terms of that protocol have now all been carried out, in three bilateral negotiations between the three pairs of countries.

Brazil-Colombia Boundary

The maximum area formerly in dispute between Brazil and Colombia is shown on Fig. 3 by the line of heavy dots. An unratified treaty of 1853, when geographical knowledge was very deficient, would have provided a boundary line approximately like the double line of dashes on Fig. 3.

The northern triple point with Venezuela is in an area that was in dispute between the three countries for nearly a century after independence; the Colombia-Venezuela boundary was determined by arbitration in 1891. From the point common with Venezuela, a 1907 treaty fixed the Brazil-Colombia boundary to the mouth of the Apaporis. The remainder of the boundary was left undecided, pending settlement of Colombia's disputes with Ecuador and Peru. Both of the latter having been subsequently settled, in 1928 the Brazil-Colombia boundary was extended southward by the Apaporis-Tabatinga line to the Amazon. That line has therefore served successively as Brazil's boundary with Peru (1851), Ecuador (1904, provisionally) and now Colombia (1928).

The complexities of each of the six preceding controversies are much greater than it has been possible to indicate in this brief summary.

Honduras-Nicaragua Boundary

Another factor that confuses the student of Latin American boundaries is the wide divergence in maps, owing largely to the lack of surveying and mapping in large areas over a long period of time.

An illustration is afforded in the area of the former dispute between Honduras and Nicaragua. Fig. 4 shows the claims of the two countries as they were presented to the arbitrator, the King of Spain. The Segovia and the Patuca rivers, which the claims followed in large part, are shown here as they appear on the millionth sheet compiled and published by the American Geographical Society of New York; these are the river courses which are followed by the symbols which are explained in the legend, on which the river names are shown.

On this map the courses of the Segovia and Patuca rivers have been added as they appeared on four of the maps which

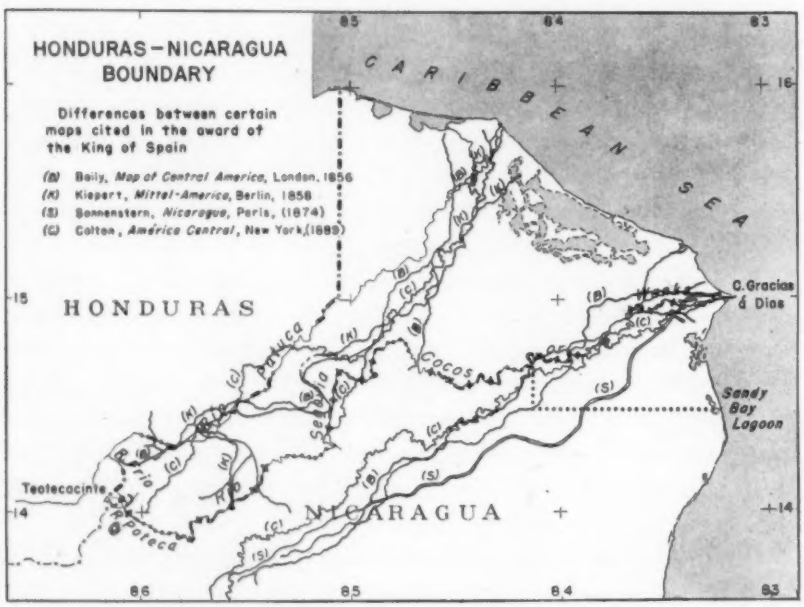

Fig. 4.

are cited in the award of the King of Spain December 23, 1906. They have, of course, been replotted on the same projection and scale. It is not difficult to imagine the confusion that would have resulted if more of the maps which were cited by the two parties to the dispute had been similarly reproduced.

A Comprehensive Plan of Study

These illustrations of problems to be faced in the study of Latin American boundaries are perhaps sufficient to indicate that there is here an ample supply of chaos from which to create a considerable world of order.

On the map of the Americas there are now 29 boundaries between Latin American republics, and 6 boundaries between a Latin American country on the one side and the United States, the Canal Zone, or a European colony on the other. There are two more boundaries between European colonies in the Guianas. No comprehensive bibliography on Latin American boundaries has been prepared. Mention should be made of a recent book on South American boundaries by Gordon Ireland.[1] An impartial, systematic history of all the boundaries in Latin America is much to be desired.

An adequate project relating to the history of Latin American boundaries would seem to require two publications:

(1) "The Map of Latin America by Treaty," a series of probably three to five volumes, containing the texts of papal bulls, Portuguese-Spanish treaties, Spanish cédulas, treaties between Latin American republics, and arbitral awards. Documents to be arranged chronologically; each in the original languages, with English translation; each to be followed by brief notes indicating its bearing on related boundary developments. Important documents to be accompanied by maps.

(2) "A History of Latin American Boundaries," probably in one volume, containing the history of each of the thirty-eight boundaries from the Rio Grande southward (including

[1] Gordon Ireland: Boundaries, Possessions and Conflicts in South America, Harvard University Press (1937), p. 126.

the provisional Brazil-Ecuador boundary, now eliminated). The boundary history of the colonial period should be given in so far as it is essential to a study of the present international boundaries and pending disputes.

The first step might well be the compilation of a list of documents which should be published and used in these two closely related publications. This list of documents might be published in a pamphlet of perhaps 80 to 100 pages, which would contain the following:

I. A chronological list of documents (papal bulls, cédulas, órdenes, treaties, arbitral awards, etc.), the publication of which is contemplated in "The Map of Latin America by Treaty."

II. A classified list of the same documents, arranged by countries, and by individual boundaries (*e.g.*, Brazil-Colombia boundary).

III. A bibliography of the treaty collections, etc., in which the larger part of the documents may be found. The bibliography should also include the more important maps, and works by individual scholars on specific boundaries.

Such a pamphlet would itself be highly useful pending the completion of definitive publications. The preparation of copy, editing, and publication of this preliminary pamphlet would probably require a year or more of research and study, and at the same time would accumulate considerable material for immediate use in the work on the major publications.

The library resources of Washington are probably unexcelled for this study—the Library of Congress, the Pan American Union, the Department of State, the Catholic University, and other collections. The map of Hispanic America in the scale of 1 : 1,000,000, nearly completed, prepared by the American Geographical Society of New York, affords an excellent cartographic base.

MATING TYPES IN PARAMECIUM AURELIA: DIVERSE CONDITIONS FOR MATING IN DIFFERENT STOCKS; OCCURRENCE, NUMBER AND INTERRELATIONS OF THE TYPES [1]

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(Communicated by H. S. Jennings)

ABSTRACT

Mating types have been found in four species of Paramecium, in addition to the two previously reported: in *P. trichium* and *P. calkinsi*, by Sonneborn, and in *P. multimicronucleatum* and *P. caudatum*, by Gilman and by Giese and Arkoosh.

Analysis of 26 stocks of *P. aurelia*, collected between North Carolina and Massachusetts and in California, shows that they are divisible into three groups, each of which consists of two mating types. Any member of one of the two types will conjugate with any member of the other, but not ordinarily with a member of its own type. The two types of group 1 are designated I and II; those of group 2 are types III and IV; those of group 3 are types V and VI. Group 1 includes 9 of the stocks; five of these (all from one source) consist exclusively of mating type I; each of the other four (from diverse sources) contains both types I and II. Group 2 includes 13 of the stocks (from diverse sources); 12 of them contain mating types III and IV, the other one has yielded type IV only. These two types give the characteristic clumping reaction and conjugate when mixed at night or early morning at temperatures not exceeding 25° C., best at 17° to 19° C. The percentage of conjugation obtained depends upon the temperature both before and during mixture of the types. Group 3 includes three of the stocks (from diverse sources); each of these contains mating types V and VI. These react and conjugate between 1 A.M. and 1 P.M. at temperatures not exceeding 27° C. Their diurnal periodicity in reactivity is a consequence of the alternation of light and darkness, visible light suppressing reactivity.

Mating types appear to be of universal occurrence in *P. aurelia;* "non-conjugating" or rarely conjugating stocks seem not to exist. Some stocks appear, however, to consist exclusively of one type, and in order to get these to conjugate, they must be mixed with the appropriate type from another stock.

A weak clumping reaction, never followed by conjugation, regularly occurs under favorable conditions in mixtures of types II and V. Hence, types I and V are similar in that both react with type II, and types II and VI are similar in that both react with type V. This suggests that these two pairs of types, and possibly all three pairs, may have originated as modifications of a single original pair of types.

[1] Aided by a grant from the Penrose Fund of the American Philosophical Society.

[2] The experimental work was carried on with the technical assistance of Miss Mildred Musher.

In *Paramecium aurelia*, there have been found (Sonneborn, 1937) two types of individuals and cultures that immediately form clumps when brought together. From these clumps, the individuals later emerge paired in conjugation, each pair consisting of one individual of one type and one of the other. As conjugation is a normal consequence of the clumping reaction and as conjugation takes place usually or only between individuals of diverse types, these types will be referred to as mating types.

Although the mating types are definite and the reaction between them striking, they had not been found in any of the many previous investigations on conjugation of *Paramecium* and other comparable ciliates. Does this mean that they are exceptional and of rare occurrence? On this question evidence is at present available only for the genus *Paramecium*. Mating types have been reported in two species, in *P. aurelia* by Sonneborn (1937) and in *P. bursaria* by Jennings (1938a). I can now report that similar types occur in two other species, *P. trichium* and *P. calkinsi*. Mr. Lauren Gilman of this laboratory, and Giese and Arkoosh of Stanford University permit me to state that they have found them in two more species, *P. multimicronucleatum* and *P. caudatum*. We have thus far found no species of *Paramecium* in which mating types are absent.

There is however a further question concerning the universality of such types. In species in which they have been found, do they occur in all stocks or only in some? According to the published reports on both *P. aurelia* and *P. bursaria*, mating types were found in some stocks and not in others. Do the latter actually lack them? In *P. aurelia*, the clue to the solution of this problem was the discovery that the species contains three kinds of stocks differing in the conditions required to bring about conjugation. An account of this will be given in the first part of this paper.

With this knowledge, the question of the universal occurrence of mating types in *P. aurelia* was investigated by examining 26 diverse stocks, including those in which such

types were earlier not found. As will appear in the second part of this paper, each of these stocks was found to consist of one or two mating types.

In order to ascertain how many *diverse* mating types exist and how they are interrelated, the 32 types found in 17 of the stocks were mixed together two at a time in all possible combinations. The results show that the 32 types are reducible to only six diverse ones. The remaining nine stocks were analyzed less extensively, but sufficiently to show that they consist of types identical with those in the stocks examined more fully. With respect to the types they contain, the 26 stocks are divisible into three groups, each group containing but two of the types. Conjugation occurs only between the two types in the same group, not between types in different groups. Nevertheless, one combination of types from different groups yields regularly a weak clumping or mating reaction; this suggests that of the six types, three may be diverse modifications of one type, three diverse modifications of another. The studies on the number and interrelations of the mating types are dealt with in the third part of this paper. Finally, the phenomena of mating types and sexuality in diverse unicellular organisms are compared and the relation of the types to sexuality is discussed.

Material. Each of the 26 stocks consists of the descendants of a single individual. A list of the stocks and their sources follows: Stock A from Strickersville, N. Y.; stocks B, F, N, O, P, T, U, V, and X from a stream in Woodstock, Md.; stocks C, J, K, M, Q, and R from different ponds in Baltimore, Md.; stock D from Counterfeiter's Lodge, N. Y.; stock E from Elkridge, Md.; stock G from near Pinehurst, N. C.; stock H from Halethorpe, Md.; stock I from near Buffalo, N. Y.; stock L from Staunton, Va.; stock S from Cold Spring Harbor, N. Y.; stock W from Williamstown, Mass.; stock Y from near Cockeysville, Md.; and stock Z from Stanford, California. For some of these stocks I am indebted to Professors H. S. Jennings, L. L. Woodruff and A. A. Schaeffer; to Drs. Coleen Fowler, R. Wichterman, D. Raffel, and A. C.

Giese; to Fathers J. A. Frisch and C. A. Berger, S. J.; and to Messrs. N. Finklestein and C. W. Metz, Jr.

I. The Conditions Required to Bring about the Mating Reaction and Conjugation in Diverse Stocks of Paramecium aurelia

With respect to the conditions required to bring about the mating reaction and conjugation, there are certain general rules that hold for all of the 26 stocks investigated. The first and most important one is that animals of appropriate different mating types must be brought together. The evidence for this will be given in the following section of this paper. Another general rule is that the individuals must not be overfed. Finally, in order to react, they must not be undergoing fission, endomixis or conjugation. In addition to these general rules, there is one that holds for a number of stocks, namely, that during the first six to ten days after conjugation the mating reaction and conjugation will not occur; but in other stocks, this period of "immaturity" is either entirely lacking or so brief as to escape detection. The existence of a period of immaturity has thus far not been correlated with any other characteristic of these stocks.

For one group of stocks (stocks B, F, J, N, O, P, R, S, T, and Z), no additional special conditions need be fulfilled in order to bring about the mating reaction and conjugation; but the remaining stocks are divisible into two groups each requiring its own particular conditions. Both groups react and conjugate only at certain hours and under certain temperature conditions, the hours and temperatures being somewhat different for the two groups.

One group, including the stocks A, C, D, E, G, H, I, K, L, U, V, W, and X, reacts best between 17° and 19° C., and not at all above 25° C. The percentages of conjugants appearing in mixtures of opposite mating types kept separately for one day and then mixed at the same temperature were 4 per cent at 9°, 68 per cent at 17.6°, 53 per cent at 18.6°, none at 24.5° or 32.2° C. Moreover, as shown in Table 1, the temperature

TABLE 1

THE NUMBER OF PAIRS OF CONJUGANTS FORMED IN MIXTURES OF 100 ANIMALS OF EACH OF THE TWO MATING TYPES IN STOCK K (GROUP 2)

		Temperature after Mixture of the Two Types				
		9.0°±0.2°	17.6°±0.2°	18.6°±0.4°	24.5°±0.5°	32.2°±0.5°
Temperature for one day before mixture	9.0°±0.2°	4	78	89	14	0
	17.6°±0.2°	14	68	72	15	0
	18.6°±0.4°	30	44	53	1	0
	24.5°±0.5°	14	36	43	0	0
	32.2°±0.5°	7	25	26	6	0

before mixture of the types, as well as the temperature after mixture, is important. The highest percentage of conjugants (89 per cent) appeared in cultures mixed at 18.6° after a preliminary day unmixed at 9° C. However, as this procedure takes three days to obtain conjugation, another more rapid technique was usually employed. The mating types were cultivated separately at 20° to 27° C., then mixed and put directly at 17° to 19° C. With this method, conjugants are ordinarily found the first morning after mixture.

When the proper temperature conditions are fulfilled, the stocks of this group give the mating reaction when opposite types are mixed at certain hours, but not when mixed at other hours. A weak mating reaction may be obtained as early as 6 P.M., though usually not until 9 or 10 P.M. Weak reactions may be obtained as late as 9 or 10 A.M., but usually not later than 7 A.M. When mixtures were made hourly throughout the night, the best immediate reactions took place between 1 A.M. and 5 A.M. The period of optimum reactivity varies by a few hours in different experiments, but it always occurs at hours so unfavorable for full investigation that I cannot claim to have analyzed this subject exhaustively. For all practical purposes it is sufficient to mix opposite types during the day

and put them at 17° to 19° C. Conjugation then begins during the night or early morning and, as pairs remain together more than 12 hours at these temperatures, the conjugants are found when the cultures are examined during the morning. In all cases where exhaustive study is required, such mixtures are examined night and morning at 12 hour intervals for two or three days.

The remaining group of stocks, including stocks M, Q, and Y, react at somewhat higher temperatures. Excellent reactions occur up to 24° and weaker ones up to 27°, but none at 30° C. Furthermore, the diurnal periodicity in this group is markedly different from that in the preceding group. The reactive period does not begin until 1 A.M. and then only feebly, and it stops completely in the early afternoon, usually before 1 P.M. The period of optimum reactivity is between 4 A.M. and 11 A.M.

With stock Q of this group, an attempt was made to discover the leading factor involved in the diurnal periodicity. As might be supposed, this factor is the daily alternation of light and darkness. Cultures kept continuously in the dark gradually start reacting earlier in the night and continue later in the day. After five days of continuous darkness, they react with equal readiness at all hours of the day and night. The opposite occurs with cultures kept illuminated continuously. (For this purpose, there was used an effective illumination of about 100 foot-candles from a water cooled 200 watt hygrade bulb, using as an infra-red filter a 1 cm. cell of N/2 copper chloride.) The reactive period gradually shortens and the reactions decrease in intensity until, after five days of continuous illumination, no further reactions occurred at any hour of the day or night. In order to ascertain whether the effective action was a stimulation by darkness, seven series of cultures were put into darkness at each hour from noon until 6 P.M. Beginning at 10 P.M. and continuing hourly thereafter, two cultures (one of each type) from each series were mixed together. None reacted until 1 A.M., and all began reacting at about the same time

regardless of when they had been put in the dark. Apparently darkness does not stimulate reactivity. In order to test the alternative possibility, suppression of reactivity by light, similar series of cultures were all put into darkness at 5 P.M. and withdrawn and tested at successive hours the following day. As appears in Table 2, the later in the day

TABLE 2

THE RELATION BETWEEN TIME OF FIRST EXPOSURE TO DAYLIGHT AND TIME OF CESSATION OF MATING REACTIVITY IN STOCK Q (GROUP 3) OF *P. aurelia*

Strong reaction denotes that 50 per cent to 100 per cent of the animals clumped; weak reaction denotes that the clumps included less than 50 per cent of the animals; negative test denotes that no clumping occurred at all. Cultures of the two mating types were brought into the light from darkness at each hour of the day and each of these was tested hourly thereafter until reactivity ceased.

Time of First Exposure to Light	Time of Last Strong Reaction	Time of Last Weak Reaction	Time of First Negative Test
8:30 A.M.	12:45 P.M.	1:51 P.M.	2:40 P.M.
9:20 A.M.	12:47 P.M.	1:48 P.M.	2:42 P.M.
10:15 A.M.	12:49 P.M.	2:52 P.M.	3:30 P.M.
11:16 A.M.	1:50 P.M.	2:50 P.M.	3:32 P.M.
12:25 P.M.	1:25 P.M.	3:45 P.M.	4:25 P.M.
1:25 P.M.	2:20 P.M.	—	3:40 P.M.
2:20 P.M.	2:45 P.M.	4:05 P.M.	4:18 P.M.
3:17 P.M.	—	3:47 P.M.	4:15 P.M.
4:15 P.M.	—	4:48 P.M.	5:15 P.M.
5:05 P.M.	—	5:45 P.M.	—

the cultures were first exposed to daylight, the later they continued to react. For example, the culture first exposed at 2:20 P.M. was still reacting strongly after the one exposed at 9:20 A.M. had completely stopped reacting; and the culture exposed at 5:05 P.M. reacted weakly four hours after the last weak reaction of the culture exposed at 9:20 A.M. Thus the ordinary daily periodicity in reactivity appears to be due primarily to the suppressing effect of light.

The foregoing account of the conditions required for the mating reaction and conjugation in the three diverse groups of stocks is intended to be little more than a portrayal of the conditions that must be fulfilled in order to carry out success-

fully investigations of the mating types. Further investigation would doubtless reveal factors influencing reactivity other than those here set forth.

II. The Universal Occurrence of Mating Types in Paramecium aurelia

To investigate the universality of occurrence of mating types in *P. aurelia*, 17 of the 26 stocks were analyzed exhaustively and the other nine less fully, but adequately. The methods employed were based on the knowledge set forth in my earlier paper (Sonneborn, 1937), that in stock S the progeny produced by fissions of a single individual are all of the same mating type so long as neither endomixis nor conjugation intervenes, but that at the first fission of an exconjugant the two types, I and II, often segregate. Kimball (1937) found that these types may also segregate at the first fission after the climax of endomixis. Therefore, every culture used in the study of mating types consisted of the vegetative progeny of a single individual produced at least one fission after an endomixis or conjugation. Such cultures include only individuals containing macronuclei descended by division from a single original macronucleus, for each exconjugant and endomictic individual forms two macronuclear anlagen that segregate at the first fission. As the individuals containing macronuclei descended from a single macronucleus are ordinarily alike in mating type, it will be convenient to introduce the term *caryonide* to denote them. A caryonide thus consists of an individual containing but one of the macronuclear anlagen formed at conjugation or endomixis and all its progeny produced until another nuclear reorganization takes place. In the rare cases when more than two macronuclear anlagen are formed at these processes, the caryonides arise at a fission slightly later than the first.

For exhaustive analysis of a stock, the method was to obtain a number of exendomictic or exconjugant caryonides and mix together samples of these in all possible combinations of two. Each mixture was then observed for the mating reaction

and conjugation. If these occurred, the cultures mixed were of different mating types. Among the stocks examined in this way, there were two (stocks B and W) in which none of the mixtures reacted or yielded conjugants; these results and their significance will be taken up later. In the remaining stocks, some of the mixtures yielded conjugants and others did not. In each of these stocks, however, the results fell into the same kind of definite system, as will now be set forth. As examples of the relations found, there will be given the results on one stock from each of the three groups differing in the conditions required for the mating reaction, for as will appear later, these groups also differ in the mating types they contain.

In group 1, in which the mating reaction occurs at any time of the day or night, the relations in stocks R and S have to some extent been reported previously (Sonneborn, 1937, 1938); stock F will be employed here as an example of the results with this group. Eleven different exconjugant caryonides were mixed in all possible combinations of two with the results shown in Table 3, where the symbol in each square indicates whether the mating reaction and conjugation occurred in mixture of the caryonides on the corresponding row and file. The symbol − indicates that no reaction or conjugation occurred, the symbol + that they did. As appears on one of the diagonals of the table, no conjugation occurred in any mixture of two cultures of the same caryonide. Among the 55 different possible mixtures of two caryonides, conjugation occurred in 30, but not in the other 25. The mixtures that gave the mating reaction and conjugation consisted of any one of the caryonides 2b1, 2b2, 3a2, 4b1 or 4b2 mixed with any one of the caryonides 1b2, 2a1, 2a2, 3a1, 5b1 or 5b2. Those that failed to react or conjugate consisted of mixtures of two of the former group of caryonides or of two of the latter group. The two groups of caryonides are thus of two different mating types: mating reactions and conjugation occur when caryonides of these two types are mixed, not when two of the same type are mixed. Thus, in this sample of stock F, only two mating types appeared. In other experiments, many

TABLE 3

RESULTS OF MIXING DIFFERENT EXCONJUGANT CARYONIDES OF STOCK F, GROUP 1, *Paramecium aurelia*

In the square formed by the junction of each row and column is a symbol indicating whether the mating reaction and conjugation occurred in the mixture of the two caryonides shown on that row and column. + signifies that they did occur, − that they did not.

CARYONIDES	2b1	2b2	3a2	4b1	4b2	1b2	2a1	2a2	3a1	5b1	5b2
2b1	−	−	−	−	−	+	+	+	+	+	+
2b2	−	−	−	−	−	+	+	+	+	+	+
3a2	−	−	−	−	−	+	+	+	+	+	+
4b1	−	−	−	−	−	+	+	+	+	+	+
4b2	−	−	−	−	−	+	+	+	+	+	+
1b2	+	+	+	+	+	−	−	−	−	−	−
2a1	+	+	+	+	+	−	−	−	−	−	−
2a2	+	+	+	+	+	−	−	−	−	−	−
3a1	+	+	+	+	+	−	−	−	−	−	−
5b1	+	+	+	+	+	−	−	−	−	−	−
5b2	+	+	+	+	+	−	−	−	−	−	−

STOCK F

other exendomictic and exconjugant caryonides were examined and all were found to react and conjugate when mixed with one or the other [1] of the two mating types indicated in Table 3. No additional or different types appeared and non-reactive caryonides were not found. Stock F thus consists of two mating types and every individual is of one type or the other. In like manner, the stocks J, R and S of group 1 were shown to consist of two mating types each.

Stock K will be used as an example of the second group of stocks—those that react at low temperatures and during the

[1] In this stock, and in other stocks of all three groups, caryonides exist which react and conjugate with both mating types of their stock. Unlike the superficially similar situation in P. bursaria (Jennings, 1938a), however, mating also occurs readily between individuals of the same caryonide. Such caryonides thus appear to contain not a third type, but both of the two types characteristic of their stock. This matter will be dealt with fully in later papers from this laboratory.

night. The stocks A, C, D, E, G, H, and L also belong to this group and show the same phenomena as those now to be set forth for stock K. In most of the work with this group, the mating reaction itself was not observed because of the inconvenient hours at which it usually occurs. Instead, as mentioned above, mixtures were made, put at 17° to 19° C. over night, and observed for conjugation the following morning. Table 4 gives the results with 16 exconjugant caryonides of stock K, using the same symbols as in Table 3. No conjugation occurred in any mixtures of two cultures of the same

TABLE 4

RESULTS OF MIXING DIFFERENT EXCONJUGANT CARYONIDES OF STOCK K, GROUP 2, *Paramecium aurelia*

In the square formed by the junction of each row and column is a symbol indicating whether conjugation occurred in the mixture of the two caryonides shown on that row and column. + signifies that it did occur; − that it did not

CARYONIDES	31a	33a	34a	35a	37b	38b	39b	41b	44b	45b	33b	34b	37a	41a	44a	45a
31a	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
33a	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
34a	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
35a	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
37b	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
38b	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
39b	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
41b	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
44b	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
45b	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
33b	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−
34b	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−
37a	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−
41a	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−
44a	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−
45a	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−

STOCK K

caryonide; but among the 120 possible different mixtures of two caryonides, conjugation took place in 60, not in the other 60. The mixtures that conjugated consisted of any one of the caryonides 31a, 33a, 34a, 35a, 37b, 38b, 39b, 41b, 44b, or 45b mixed with any one of the caryonides 33b, 34b, 37a, 41a, 44a, or 45a. Those that failed to conjugate were mixtures of two caryonides of the former group or two of the latter. The caryonides of one group are thus of one mating type and those of the other group are of another mating type, and conjugation occurs only when the two types are mixed. In stock K, as in stock F, only two mating types appeared, for all of the hundreds of exendomictic and exconjugant caryonides examined in other experiments conjugated when mixed with one or the other of the two types shown in Table 4.

Similar results were obtained with the third group of stocks (M, Q, and Y) which react between 1 A.M. and 1 P.M. Stock Q will be used as an example of this group; identical results were obtained with the other two stocks M and Y. Table 5 gives the results of an analysis of 17 exconjugant caryonides of stock Q. No conjugation occurred in any mixtures of two cultures of the same caryonide, but the 136 possible different mixtures of two caryonides showed that two mating types were present: eight caryonides were of one type and nine of the other. The 72 different mixtures consisting of two caryonides of different type all conjugated; the 64 different mixtures consisting of two caryonides of the same type did not. In further studies on hundreds of other exconjugant and exendomictic caryonides of stock Q, the same two types constantly reappeared and no new ones: all conjugated when mixed with one or the other of the two types shown in Table 5.

Thus, each of these 15 stocks, belonging to three diverse groups, consists of two mating types. The remaining two stocks, B and W, are different. In each of them, hundreds of exendomictic caryonides were mixed together under conditions favorable for conjugation, but not one pair of conjugants ever appeared. Are these "non-conjugating" stocks? Or do they require for conjugation conditions different from those re-

TABLE 5

RESULTS OF MIXING DIFFERENT EXCONJUGANT CARYONIDES OF STOCK Q, GROUP 3, *Paramecium aurelia*

In the square formed by the junction of each row and column is a symbol indicating whether the mating reaction and conjugation occurred in the mixture of the two caryonides shown on that row and column. + signifies that they did occur; − that they did not.

CARYONIDES	2b1	3b1	4b1	5b1	7b2	13b1	16a2	30a2	1b2	2b2	3b2	4b2	5b2	7b1	13a2	19b1	21b2
2b1	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
3b1	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
4b1	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
5b1	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
7b2	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
13b1	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
16a2	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
30a2	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+
1b2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
2b2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
3b2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
4b2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
5b2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
7b1	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
13a2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
19b1	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
21b2	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−

STOCK Q

quired by the three groups of stocks already described? Or does each of these stocks consist of but one mating type?

The problem was easily solved by mixing caryonides of stocks B and W with caryonides of other stocks. Stock B gave the mating reaction and conjugated when mixed with type II from stocks R or S, but not when mixed with type I. Stock W invariably conjugated when mixed with one of the two mating types of stock K, but not when mixed with the

other. Thus, stocks B and W are not "non-conjugating" stocks and do not require conditions for conjugation other than those already set forth; moreover, each does contain at least one mating type.

In order to discover whether in each of these stocks all the caryonides were of the same mating type, each caryonide derived from a group of exendomictic individuals was separately tested. Of 75 exendomictic caryonides of stock B, 28 were non-viable and the remaining 47 all reacted with type II of stock R. All the viable caryonides were therefore of type I. Of a group of 81 exendomictic caryonides of stock W, 76 were viable and all reacted with one of the two types in stock K, none with the other type. It thus appears that in each of the stocks B and W all of the viable caryonides are of one mating type.

It is of interest that one of these stocks, W, is the famous long-lived stock cultivated since 1907 by Woodruff (1932). Woodruff (1921) has twice obtained conjugation in it. This suggests that it sometimes gives rise to the other mating type found in stock K; but this has not yet happened during my four years of observations on it.

After these unusual results were obtained with stocks B and W, new collections were made from the stream in Woodstock, Md., from which stock B had been obtained. Four individuals obtained from one quiet pocket of the stream gave rise to the four stocks N, O, P, and T. In each stock, endomixis was induced and the exendomictic caryonides tested. In every case, they failed to react with each other or with type I, but did react and conjugate with type II from stocks R or S. They were thus all like stock B, consisting exclusively of type I. However, not all stocks from this stream are like this, for three other individuals collected from another pocket of the stream gave rise to the stocks U, V, and X which belonged to group 2 and contained two types each.

The general result of this survey of 24 stocks of *P. aurelia* is thus to show that all consist of mating types, most of them of two types that react and conjugate with each other, but some

of them apparently consisting mainly or exclusively of only one type. If these 24 stocks, collected from North Carolina to Massachusetts, are a fair sample of the species, it can be concluded that "non-conjugating" or even "rarely conjugating" stocks do not exist: all conjugate regularly when the appropriate mating types are mixed together under appropriate conditions. Mating types thus appear to be of universal occurrence in *P. aurelia*. Previous failures to observe them must be ascribed not to their absence, but to failure to grow the types separately and to mix them together in proper combinations under the necessary conditions.

III. The Number of Diverse Mating Types in P. aurelia and their Interrelations

The questions now arise as to whether the mating types in different stocks are alike or not; how many diverse types exist; and how they are interrelated. In order to answer these questions, all possible combinations of two were made among the stocks B and W and the two types from each of the other 15 original stocks. These 496 different mixtures were made with the results shown in Table 6. In 395 of the mixtures conjugation did not occur; in the remaining 101, it did. The mixtures in which conjugation occurred are classifiable into three groups in the following way.

The first group involves the stocks B, F, J, R, and S. The two types in stocks R and S are the mating types I and II, as set forth in my earlier papers (Sonneborn, 1937, 1938). Stock B consists of type I only, as shown in the preceding section. Of the two types found in each of the stocks F and J, one is type I, for it conjugates with type II and not with type I from the other stocks; and the other is type II, for it conjugates with type I but not with type II from the other stocks. Among these five stocks, conjugation occurred when type I from any of the five was mixed, in any of the 20 possible different combinations, with type II from any of the four stocks in which it occurs. The remaining 16 different combinations among these stocks involved mixtures of two cul-

tures of the same type and no conjugation occurred. Moreover, no conjugation occurred in any of the 207 possible mixtures of either type I or type II from these stocks with any of the types found in the remaining 12 stocks. Hence, neither type I nor type II occurs in those stocks.

The second group of mixtures yielding conjugation involved the nine stocks A, C, D, E, G, H, K, L, and W. As types I and II do not occur in these stocks, the two types in any one of them, stock A for example, require new designations. They will be called mating types III and IV. That the other stocks of this group contain the same types is shown in Table 6 by the results of mixing them with types III and IV of stock A. Thus stock W conjugates in mixtures with type III from stock A, but not with type IV; therefore, the one type existing in this stock must be type IV. In each of the other seven stocks of this group, one type conjugates in mixtures with type III from stock A, but not with type IV; the other conjugates in mixtures with type IV from stock A, but not with type III. The former is thus type IV, the latter type III. In agreement with this, each of the possible 72 different combinations between types III and IV from different stocks yielded conjugants, whereas none were found in any of the 64 possible combinations between two cultures of type III or two of type IV. Types III and IV in these nine stocks not only failed to conjugate in mixtures with types I and II, but also in all of the 102 possible mixtures with the two types in the remaining three stocks. The latter, therefore, do not contain types I, II, III, or IV.

The third group of mixtures yielding conjugation involved combinations among the types in these three remaining stocks. As the four types previously discussed do not occur in them, the two found in any one stock—M, for example—will be designated mating types V and VI. As Table 6 shows, the two types in stocks Q and Y must also be types V and VI; for one conjugates with type VI, but not V; and the other conjugates with V, but not with VI from stock M. In agreement with this, conjugation occurred in each of the six possible

TABLE 6

RESULTS OF MIXING IN ALL POSSIBLE COMBINATIONS OF TWO THE 32 MATING TYPES FOUND IN 17 DIFFERENT STOCKS OF *Paramecium aurelia*

In the square formed by the junction of each row and column is a symbol indicating whether conjugation occurred in the mixture of the two types shown on that row and column. + signifies that it did occur; − that it did not. The capital letters give the names of the stocks, the Roman numerals designate the mating types.

		GROUP 1								GROUP 2																GROUP 3					
		B I	F I	J I	R I	S I	F II	J II	R II	S II	A III	C III	D III	E III	G III	H III	K III	L III	A IV	C IV	D IV	E IV	G IV	H IV	K IV	L IV	W IV	M V	O V	Y V	M VI	O VI	Y VI
GROUP 1	B I	−	−	−	−	−	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	F I	−	−	−	−	−	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	J I	−	−	−	−	−	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	R I	−	−	−	−	−	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	S I	−	−	−	−	−	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	F II	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	J II	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	R II	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	S II	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
GROUP 2	A III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	C III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	D III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	E III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	G III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	H III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	K III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	L III	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−
	A IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	C IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	D IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	E IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	G IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	H IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	K IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	L IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
	W IV	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−
GROUP 3	M V	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+
	O V	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+
	Y V	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+
	M VI	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	−	−	−
	O VI	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	−	−	−
	Y VI	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	−	−	−

combinations of type V with type VI from different stocks, but not in any of the six possible combinations of two type V cultures or two type VI cultures from different stocks.

The types in the remaining nine stocks were not tested with all the other stocks, as Table 6 shows this is unnecessary. All that is required to ascertain the types in a new stock is to

test them against one standard culture of each of the six diverse mating types, for the behavior of any type is consistent: if it reacts with one type from any one stock, it will react with this type from any other stock; and if it fails to react with one type, it will not react with this type from any stock. Using this method of identification, it was found that the stocks N, O, P, and T were of type I, because they reacted and conjugated with type II, but not with any other type. In like manner, the two types found in stock Z were found to be types I and II. And those in the stocks I, U, V, and X were types III and IV, for one type from each of these stocks reacted and conjugated with type IV, but not with any other type; and the other reacted and conjugated with type III, but not with any other.

The 26 stocks are thus divisible into three groups on the basis of the mating types they contain. In the first group are stocks containing mating types I or I and II; these are the ten stocks B, F, J, N, O, P, R, S, T, and Z. Stocks of the second group contain mating types IV or III and IV; they are the 13 stocks A, C, D, E, G, H, I, K, L, U, V, W, and X. In the third group are stocks containing mating types V and VI; these are the three stocks M, Q, and Y. This grouping of the stocks on the basis of the mating types they contain gives the same results as grouping them on the basis of the environmental conditions required to get the types to react and conjugate. Types I and II, constituting group 1, react at any hour of the day or night over the entire range of temperatures tested (9° to 32° C.). Types III and IV, constituting group 2, react at night below 25° C. Types V and VI, constituting group 3, react below 28° between 1 A.M. and 1 P.M. Thus, the three groups of stocks differ not only in the mating types they contain, but also in the conditions required for conjugation.

The failure to obtain conjugation between stocks of different groups is not, however, due to the differences in external conditions required for conjugation. Types I and II react under conditions favorable for any of the other types; and the

types of groups 2 and 3 all react between 1 A.M. and 9 A.M. at low temperatures. The mixtures between stocks of different groups were always made under conditions favorable for both groups involved. Their failure to yield conjugants must thus have been due to internal, not external, blocks to union.

From the observations set forth up to this point, the mating system in *P. aurelia* can be formulated in four propositions. 1.—The species consists of three groups of stocks differing in mating types and in the conditions required for conjugation. 2.—Each group includes only two mating types. 3.—Each stock consists of one or both of the mating types characteristic of its group. 4.—Conjugation occurs only between the two mating types of the same group, so that the three groups do not interbreed. These mating relations of the different types and groups are summarized in Table 7. They show on a

TABLE 7

RESULTS OF MIXING THE SIX MATING TYPES OF *Paramecium aurelia*

+ signifies that conjugation occurs in the mixture of the two types on the corresponding row and column; − that it does not. The six types fall into three groups of two types each, with conjugation only between the two types of the same group.

			Group					
			1		2		3	
		Type	I	II	III	IV	V	VI
Group	1	I	−	+	−	−	−	−
		II	+	−	−	−	−	−
	2	III	−	−	−	+	−	−
		IV	−	−	+	−	−	−
	3	V	−	−	−	−	−	+
		VI	−	−	−	−	+	−

larger scale a difference between *P. aurelia* and *P. bursaria* that appeared when only one stock of *P. aurelia* had been examined. For, in *P. bursaria*, the mating types occur in groups of four

or more with conjugation taking place between any two of the four or more types of a group (Jennings, 1938a and b); whereas, in *P. aurelia*, there are invariably two mating types to a group and conjugation occurs between them only.

The sharp division of the species *P. aurelia* into three groups of non-interbreeding stocks raises the question of whether these different groups are different species. All these groups conform to the specific description of *P. aurelia*, possessing two vesicular micronuclei. Within each group of stocks there are, under certain conditions, slight differences in body size and form; but consistent morphological characters differentiating all the stocks of one group from all of those of another, have not been found. The three groups do differ consistently in the mating types they contain and in the conditions required for conjugation. On the basis of the latter, the group to which a new stock belongs could be determined without testing against the six standard types. If conjugation occurred readily above 27° C. and equally well at all hours of the day and night, the stock belongs to group 1; if it occurred only below 25° and only at night or early morning, the stock belongs to group 2; if it occurred only below 27°, and only between 1 A.M. and 1 P.M., the stock belongs to group 3. As the different groups of stocks do not interbreed, they would be considered by some (Dobzhansky, 1937) to be separate species. Practically, however, it would require observations of such a special kind to ascertain the species that it seems inexpedient for the present at least to set up these three groups of stocks as separate species.

The mating reaction and conjugation have thus far been dealt with as if they were invariably correlated. This is, in fact, the usual condition: conjugation ordinarily follows the mating reaction. In at least one combination of mating types, however, a weak but definite clumping reaction regularly occurs under appropriate conditions and is never followed by conjugation. This reaction appears between 1 A.M. and 1 P.M. at temperatures below 27° C. in mixtures of type II with type V; but only when the two cultures mixed are in the most

favorable condition for conjugating, that is, when each gives immediate strong reactions in mixtures with the other type of its own group. The same result is obtained regardless of which stocks are used as sources for the two types: type II from any of the five stocks F, J, R, S, or Z in which it exists reacts with type V from any of the three stocks M, Q, or Y in which it exists. In all these mixtures there are formed pairs or small clusters that go through the characteristic early stages of the mating reaction and then break apart with a sharp jerk. That the reaction involves animals of both types is evident when mixtures are made between small type II and larger type V animals, or vice versa: the unions are invariably between animals of diverse sizes. In mixtures of about 50 animals of each type, usually only one or two pairs or small clusters are present at any one time and these remain together for only a minute or less. There are frequently intervals of one or a few minutes in which no pairs are present, but new pairs form repeatedly throughout the ordinary period of reactivity of group 3. After this period the reaction ceases; but if the mixture is kept until the following day, the reaction is repeated during the forenoon. During the period of reactivity, the behavior is such as takes place near the end of the daily reactive period in mixtures of type V and type VI, when temporary pairs or clusters form for a short time and break apart without conjugating.

This type of reaction has not been found in any other combination of mating types belonging to different groups. If it existed, one would expect to observe it most favorably between 1 A.M. and 5 A.M. in the case of mixtures between groups 1 and 2, and between 4 A.M. and 5 A.M. in the case of mixtures between groups 2 and 3. In the several trials made at these hours, the results were completely negative. The occurrence of clumping without conjugation in certain mixtures of types belonging to different groups and not in others appears to resemble the results of Strehlow (1929) on the alga Chlorogonium. When + and − gametes of the two species *C. euchlorum* and *C. elongatum* were mixed, in certain

interspecific combinations, the gametes adhered to each other, in other combinations not, and in no case were zygotes formed. The regular occurrence of the clumping reaction between types II and V, which do not interbreed, suggests that the four types I, II, V, and VI include two modifications of one original type and two of another. For types I and V are similar in that both react with type II, and types II and VI are similar in that both react with type V. In agreement with this, other similarities between types I and V, on the one hand, and between types II and VI, on the other, have been found. These similarities suggest common origins; namely, that two, and possibly all three, of the pairs of mating types may be diverse modifications of a single original pair of types. Full discussion of this question will be given in a later communication, after other pertinent evidence has been presented.

Do the three pairs of mating types reported above include all the types existing in *P. aurelia?* As only 26 stocks have been examined, and as all but one of these (the one from California) come from the eastern United States between North Carolina and Massachusetts, it is possible that further search, particularly in other localities, will turn up more pairs of types. I should be glad to receive and analyze cultures collected elsewhere.

IV. The Relation of Mating Types of Paramecium to Sexuality

The recent work on mating types in *Paramecium* has demonstrated among the conjugants the existence of physiological diversities that are strikingly similar to the sexual differences between gametes in some of the unicellular green algæ (Strehlow, 1929; Moewus 1933, 1935). In the latter, cells capable of uniting in copulation are of two types, plus and minus; when cultures of these two types are brought together under appropriate conditions, they at once form clumps ("Gruppenbildung") and proceed to copulate. The plus and minus gametes are morphologically and functionally alike; but, as copulation takes place only between cells of dif-

ferent types, they are physiologically diverse. In these respects, therefore, the plus and minus sexes in the unicellular algæ are like the mating types of *Paramecium*.

There are, however, certain differences between the phenomena in the two groups of organisms. In the algæ, the two sexes have been shown (Moewus, 1933) to elaborate specific "sex stuffs" which can be obtained from the fluid they have lived in. Attempts to demonstrate such "stuffs" in *Paramecium* have not been successful. A further difference exists in the number of kinds of mating classes. In the algæ, there are always only two kinds, though each kind may exist in varying strengths; in *P. aurelia* there are likewise but two kinds in any mating system, but in *P. bursaria* (Jennings, 1938a and b), there may be four or five different mating types in one system. Finally, in *Paramecium* all the mating types are functionally hermaphroditic, each conjugant of a pair fertilizing the other one, while in the unicellular algæ reciprocal fertilization does not occur.

In view of these differences, it has been held that the similarities between the phenomena in the unicellular algæ and in *Paramecium* are not indicative of fundamentally similar processes; indeed, that in the former they are to be considered sexual phenomena, in the latter, not. The chief fact held to show that the phenomena of mating types in *Paramecium* are not sexual is the hermaphroditic condition of all the types. As each conjugant produces both a "male" and "female" pronucleus, the conjugant as a whole cannot be male or female. On the other hand, there is evidence in other ciliates that vestiges of the hermaphroditic condition can coexist with individual sex differences. In the Vorticellidæ, the conjugants are markedly dimorphic as well as functionally diverse: the microconjugant is motile and fertilizes the sessile macroconjugant. Yet, in both conjugants two pronuclei are formed as in the ciliates with morphologically identical conjugants and reciprocal fertilization. Moreover, in one of the Vorticellidæ, *Zoöthamnium arbuscula*, Furssenko (1929) gives some evidence that even reciprocal fertilization may occur,

though followed by disintegration of the syncaryon in the microconjugant. It seems, therefore, that hermaphroditism need not be completely incompatible with other aspects of sexual diversity. With respect to that aspect of sex concerned with fertilization itself, the two cells that come together may be hermaphroditic; while with respect to that aspect of sex concerned with bringing together the cells, they may be diverse. That is the situation in *Paramecium*. However, opinions may differ as to whether processes concerned simply with bringing cells together for mating are in themselves sexual. It appears desirable, in the present state of knowledge, to focus attention on the facts rather than on their interpretation. Therefore, the interpretive terms "sexes," "sex types," and "sex reaction types" used in previous papers have been replaced in this paper by the purely descriptive term "mating types."

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THE TRANS NEPTUNIAN PLANET SEARCH*

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(*Read, April 21, 1938*)

THE ninth planet, Pluto, was found early in 1930, terminating the long search for Lowell's mathematically predicted Planet X. Final success, after years spent in searching with inferior instrumental means, came in scarcely a year after the completion of the specially designed search telescope—indeed the tenth search plate made with this powerful instrument recorded the planet although it was not until the examinations of plates of the following season that Mr. Tombaugh actually found the planet. Then quite naturally there appeared to us weighty reasons why the Lowell Observatory should proceed with the larger campaign of searching a wide ecliptic belt of the heavens for still more distant members of the solar system. There were reasons for expecting such bodies might exist. For example Pluto with his smallness compared with his neighbors, Neptune and Uranus, suggested striking similarity now of the Solar planet system to that of the Jovian satellite system in that both systems possess, we see, smaller order of bodies in their outer portions and there is then the intimation that we may expect to find other more distant planets of Pluto's order of size.

Furthermore we believed the search could be well done because of very favorable surrounding circumstances: good skies, very efficient instrumental means, perfected methods, and an enthused observer of high technical skill and experience. Yet we from experience realized the task was not an easy one because it involved searching a wide area of the heavens and the careful testing of many millions of stars for planetary motion; but the search needed to be made.

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Perhaps a little ought here to be said of the manner of such searching. Fortunately the observer rides the swiftly moving Earth which by its orbital motion carries him past any distant planet causing it to appear to shift from night to night seen against the far background of fixed stars. The procedure is then to photograph the identical region of sky, using the same guide star, on two different plates two or a few nights apart, for then the distant planet will be a little shifted on the two plates. Thus the observer continues to make such matched pairs of plates of the sky until all of it is covered which is to be surveyed. The observer makes these pairs of duplicate plates as perfect as it is possible to make them, and especially must he keep them free of dust. This is most important for it takes ten times as long to examine the plates as it does to make them, and the tiny speck of dust that looks like a star image can cost much time and trouble in searching the plates.

The two members of these pairs of duplicate plates are then placed under the two microscopes of that most efficient Blink comparison machine, and the observer proceeds to examine them until every pair of the many thousands of star images on the plates have been carefully tested for motion, because it is only by shift motion that we could recognize such a distant planet. Since furthermore we can expect, in the whole sky, only one (or possibly two) such planets the observer must be ever alert lest he pass over the coveted little planetary images. He must not make even one miss. Such examination must be far more precise and painstaking than if he were looking for proper motion stars, variable stars or the like. He, as a matter of fact, sees the numerous variable stars, asteroids and nebulæ, etc., which in passing he marks and lists as by-products of the survey.

Since 1930, except for some time at the University, Mr. Tombaugh has given all his time to this search. He has pushed it forward with praiseworthy care, energy, and skill. Most of the time during the last 3 or 4 years he has been able to devote all his energy to the critical examination

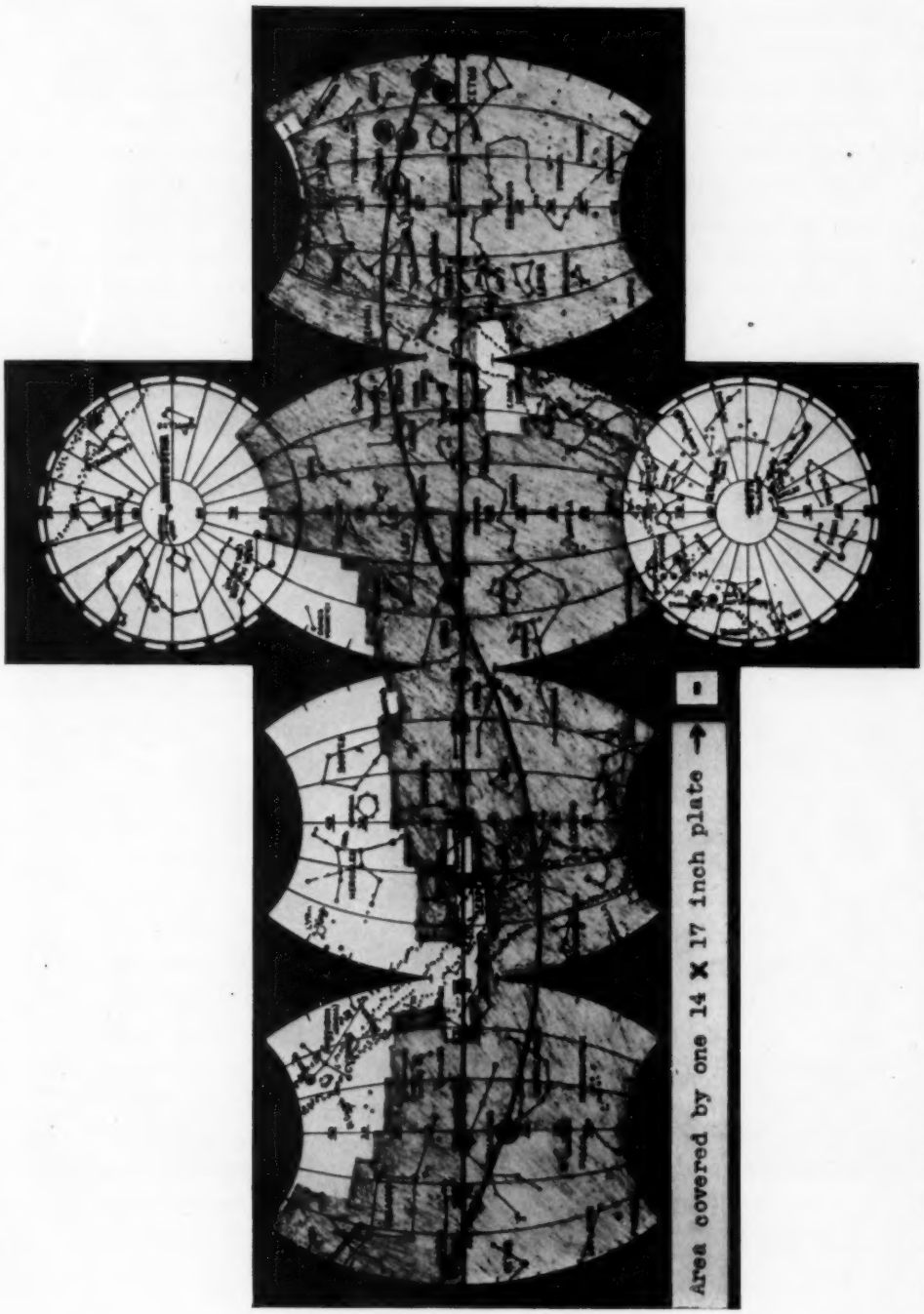

FIG. 1. The shaded portions represent the area of the sky which has been carefully examined (to April 1, 1938). Heavy, curved line represents the plane of the Earth's orbit, "*The Ecliptic.*"

of the plates, for a technical assistant has been since then available to make the exposures of the plates at the telescope. This has shortened the time and lessened therefore the overlap of plates, which much reduces the total work.

Thus far Mr. Tombaugh has carefully examined every star for planetary motion in 27,000 square degrees, or sixty-five per cent of the whole sky. The illustration in Fig. 1 shows a map of the whole sky, of which the shaded part denotes the portions of the heavens that have been examined to April 1. The heavy curved line is the ecliptic, which is the most likely portion of the sky for planets, although the high inclination of Pluto's orbit and of those of some of the outer satellites of Jupiter and Saturn suggested it would be wise to extend the search also to considerable distances north and south of the ecliptic. Below, midway in this figure is a small white rectangle which denotes the size of one of the 14×17 inch search plates on the scale of this map of the heavens, and on this white rectangle is a tiny black spot of just the size to include five square degrees of sky. Within this small area of a plate in the Milky Way east of Sirus were counted 15,000 stars on one of the original plates. While on the average the stars are not nearly as numerous as they are in the Galaxy still the number of stars thus far examined for planetary motion amounts to about 35,000,000. The total area covered is 8,000 times that represented by this tiny spot; counting the needed over-lap of the plates, and the examination of the duplicate plates, the survey has included about 70,000,000 star images to date.

In the course of the work a number of suspected planetary images were met, and a few of these seemed very promising, but in running them down all these proved to be false leads of spurious tiny specks that perfectly simulated real planetary images. Some of these involved the securing of additional check plates in order to decide that they were not true images, for nothing could be taken for granted with such cases.

The result then is that no outer planet has yet been found by this search beyond Pluto. Still it is well that we have

this knowledge of the solar system, even though the result in that sense happens to be negative. There appears to be no outer planet as bright as the sixteenth photographic magnitude for the reach of the survey is to the 16th stellar magnitude a little less in the lowest southern declinations and a little more in the higher declinations. The search has been thorough and homogeneous with standardized plates and methods. It is hoped in a few months to complete the few unfinished areas when the sky will be covered from near 50 degrees S. to 60 degrees N. declination for most of the northern sky.

Mention has been made of byproducts of the search, and a few comments and slides touching these matters may be of interest. The asteroids, those little bodies belonging mainly between the orbits of Mars and Jupiter, move rapidly and so leave their trails on these plates. More than 3,000 asteroids have been recorded on the plates and their trails

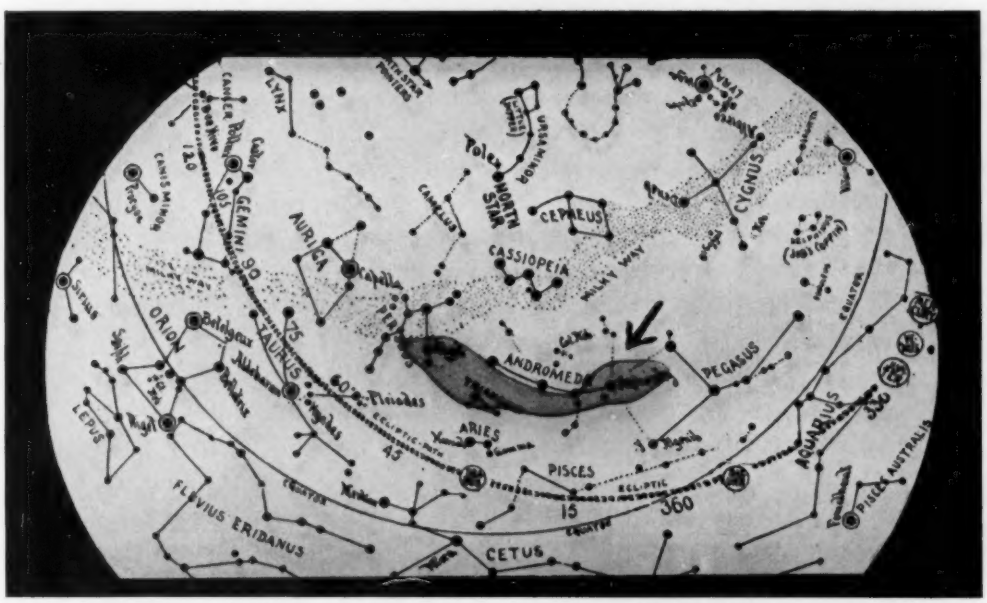

FIG. 2. The arrow points to the shaded portion, which indicates the position and extent of the "*Great Perseus-Andromeda Stratum of Nebulæ*," discovered in the planet search.

marked. The asteroids have of course been observed to become rare in those parts of the sky at considerable distances from the ecliptic. The positions of the asteroid images on these plates are being measured and published because such information is much desired by those investigating this remarkable and multitudinous group of bodies. While the majority of these images are the record of known objects there will be found among them many new asteroids and some known ones for which more data are much to be desired.

It is estimated that more than 30,000 nebulæ are recorded by these search plates and a full record has been kept of them, and some study has been made of their distribution over the sky, counts having been made of their numbers in selected areas of the heavens. In the autumn of 1936 Mr. Tombaugh came upon a remarkable rich cloud of nebulæ, which is illustrated in the shaded region of Fig. 2 of the sky map. This grouping of nebulæ is in Perseus and Andromeda, covers about 300 square degrees, and includes 1,800 individual objects. All are small and rather faint, indicating they are distant and a remarkable aggregation of island universes.

Of course numerous variable stars are also recorded. These naturally are of short period, *i.e.* those that change appreciably in two or three days. Some of these are of exceptional interest and new. They are marked on the plates and also listed in the search records.

This extensive and uniform library of sky photographs will have other values than those indicated here and should be fruitful in later years in other studies.

In conclusion, it need hardly be stated in the face of such evidence as has been given that Mr. Tombaugh merits high praise for the keen interest, skill, and untiring energy with which he has carried forward this search. Also for making many excellent search plates credit is due Messrs. Frank K. Edmondson and Henry L. Giclas. And finally thanks are due to the American Philosophical Society for its aid of two grants from its Penrose Fund.

THE PLANETS FROM OBSERVATIONS AT THE LOWELL OBSERVATORY

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(Communicated by V. M. Slipher)

(*Read April 21, 1938*)

ABSTRACT

This observatory has emphasized the study of the planets since 1894, and the present paper briefly discusses the observations of Mercury, Venus, Mars, Jupiter, and Saturn, and their atmospheres. Direct photographs portray the general appearance of these planets, significant changes, as well as examples of tell-tale phenomena observed during the past thirty years.

Mercury appears as atmosphereless and unchangeable as our moon and, keeping always the same face to the sun, thus is completely desolated by a surface temperature of 280° to 400° C.

Venus' true surface is hidden by a considerable atmosphere, rendered virtually opaque by some medium, probably dust, and hence her day is not yet definitely determined but is long. Her atmosphere contains 400 to 500 meter atmospheres of CO_2, but oxygen and water have not yet been detected. The temperature of her actual surface is more difficult to determine than Mercury's, but appears about 50° C.

Mars is analogous to the Earth, and its surface is marked by polar snow-caps and blue-green areas which change with its seasons, has a moderate surface temperature, and displays an atmosphere of considerable extent which supports clouds at relatively great heights and scatters light to an amazing degree, but is very changeable in the latter respect.

Jupiter presents a belted surface of mobile, changeable, and vari-colored clouds of unknown composition, but not of water-vapor. The surface temperature is low—near −120° C., but the great activity of his surface indicates a fairly hot interior. His overlying atmosphere, containing CH_4 and NH_3, is very transparent although of a molecular population comparable with the Earth's.

Saturn closely resembles Jupiter, but its surface displays less activity and less conspicuous markings except in rare instances such as the great white spots of 1875 and 1933. The surface temperature is near −150° C., and the composition and behavior of its upper atmosphere simulates Jupiter's.

THE special aim of the Lowell Observatory since its founding has been the study of the planets of our solar system, beginning with their present state and looking thence to their evolutionary history. To accomplish this purpose it has been necessary to collect information, as accurate and

extensive as possible, by well tested means and methods, and to plan the program in ways to secure data which bear upon the various phases of the problem. To this end the program at Flagstaff has included in addition to visual and photographic studies long continued spectrum studies and more recently radiometric observations.

It is intended here to consider chiefly results obtained from the visual and photographic observations of the surfaces and atmospheres of Mercury, Venus, Mars, Jupiter, and Saturn.

Diverse as the planets are today, they are all representative of certain evolutional processes and so knowledge of one member may throw light upon the development of others. Therefore, as intimate knowledge as possible of the several planets is important. Thus they have been closely studied visually at this Observatory for more than forty years, and during the last third of a century the visual observations have been supplemented by direct photographs. The photographic record has been kept as homogeneous and as complete as possible so as to afford a permanent record with which future results may be compared. Early experience has shown that photographs of the planets in light of different colors were valuable aids in their study. Thus for more than fifteen years we have kept a fairly complete record of the several planets in blue light, simultaneous with the usual series in yellow light. And for some years these series have been supplemented and extended by photographic studies in ultra-violet and infra-red light. Indeed, advantage has always been made of any new means or methods that afforded any new approaches to these planetary investigations.

The camera system as employed on the 24-inch refractor magnifies the planet about 200 times on the original negatives. The exposure times vary with the planet, plate, and filter, but generally range from 1 to 60 seconds.

A very large number of photographs totaling about 500,000 images of the planets has been secured during the past thirty-five years. Comparison of photographs taken at

different times bring out many interesting and instructive features and phenomena that might otherwise be missed or less well recorded.

It will be understood that because of the graininess of plates not much of the delicate detail of the original photographs can be reproduced with a projecting lantern. This is unfortunate because in the fine detail of the plates is often the most instructive part of their record.

Mercury

The markings on Mercury are faint, diffuse, and irregular in form and arrangement. They are of a straw-colored grey, and bear the look of soil or rock. These facts and his low albedo imply that we see simply barren rock or sand weathered by eons of exposure to the hot Sun. There is no distinctive color in any part of the planet. He has no polar caps, and the fixity of the markings affords evidence that the planet's rotation period is the same as its period of revolution. The visual and photographic observations reveal no evidence of any clouds, *i.e.* of any sensible atmosphere. His surface presents as dead an appearance as does that of our own Moon. With no air to mitigate the intense heat of the sun which is seven times as intense as the Earth receives, its surface temperature, as measured with the thermocouple is high—at aphelion 280° and over 400° C. at perihelion—a temperature sufficient to melt such metals as lead and zinc. Such intense heat would have speedily dissipated his original atmospheric envelope. Thus, one imagines the desolation of his surface is complete.

Venus

Venus, being relatively near us, displays a correspondingly large disk, but one which has proved singularly baffling. An air-enshrouded planet we know her to be, by her high albedo, by the observed prolongation of the horns of her crescent beyond the geometric half circle, and by the variable atmospheric spots on her ultra-violet photographs. It is this

very atmosphere that prevents us seeing her actual surface clearly (Pl. I). Thus she is difficult of observation. The length of her day is not satisfactorily determined. Still from spectrographic and other evidence her diurnal turning—her rotation period—is doubtless very slow but whether her day and year are exactly equal there remains some doubt. It is hoped to gain further knowledge of her period of rotation from the markings on the ultra-violet photographs. But these as yet have proved too ephemeral to indicate rotation (Pl. II).

Spectrographic investigations at Flagstaff and elsewhere of Venus have failed to reveal water vapor or oxygen in her atmosphere; but Dunham at Mt. Wilson discovered carbon dioxide to be present, and the quantity has been determined by Adel at Flagstaff as quite large, nearly 500 meters at atmospheric pressure. However, something more and different is required to account for the high reflectivity of her atmosphere which gives her a high albedo and prevents us from seeing her surface. It now seems more probable than ever that this something is dust, as suggested long since by Lowell. A dust laden atmosphere would appear to account satisfactorily for all the observed facts.

Venus, situated about seven-tenths our own distance from the sun, gets about double our supply of solar radiation and her surface temperature is relatively higher than the Earth's. The temperature of her true surface cannot be so definitely stated as in the case of Mercury, but an effective temperature near freezing for the integrated radiation measured radiometrically is indicated for her radiating layer. Analysis of band spectra by Adel indicates a surface temperature higher than 50° C.

Mars

It is a fortunate circumstance that the planet Mars, the one most favorably situated for observational study should duplicate so closely our Earth in seasonal effects, intake of solar radiation, and in other respects as will appear in what is to follow.

In case of Mars we see a solid surface and deal with a geography and meteorology as real as our own. Quite unlike the markings upon Jupiter and Saturn, where all we see is cloud, in the definite markings on Mars we gaze upon the actual surface features of his globe. His disk we see capped by cloud and snow at the poles, in season, with the intervening area divided between ochre colored desert, and dark blue-green regions which seasonally behave as vegetation. Both the polar caps and the blue-green areas are observed to wax and wane in precise step with his seasons. Then there are other changes in some dark markings which do not obey the seasons, but are of a secular nature. We find him encompassed by an atmosphere of considerable extent bearing clouds and haze some of which are yellowish and others bluish white in tint. Besides, his atmosphere manifests a most amazing influence on light, an influence perhaps out of all proportion to the density of it, being usually so opaque to the short waves as to obliterate all the surface markings, whereas this same blue light we have seen to pass readily through the atmospheres of Jupiter and Saturn without noticeable effect. Yet at other times, his air clears up in some strange manner so as to become nearly transparent to this very same light, as we shall now see in the photographs.

Plate III shows photographs of Mars displaying seasonal change in his polar snow caps and in the blue green areas from Martian date (upper hemisphere) March 2 to August 21. With the advance of summer his snow cap dwindles and the dark regions increase in intensity. This is the usual seasonal behavior, and in the main repeats itself every Martian year.

Plate IV shows independent drawings of Mars by Trumpler (at left) at Lick Observatory and by the writer at Flagstaff. About 50 canals, oases, and other fine details are common to both observers and they agree substantially as to the position and fineness of the canals and the other features.

Plate V is a group of drawings and photographs displaying the life-history of one of the most remarkable storms ever seen at midday on Mars. Figures 1 and 2 show the normal

aspect prior to the storm. The succeeding images display the storm as a bright spot, covering 300,000 square miles on July 9; it changes in its position, size, and form strikingly by July 10, and on the 12th has nearly disappeared. Such striking storms are very rare on Mars, especially in the tropics and at Martian noonday.

Plate VI shows photographs of the same face of Mars taken at different times, displaying remarkable changes in the clarity of his atmosphere. The first image, Fig. 1, is in red light; all the others are in blue light and display various degrees of cloudiness, of general opacity, and of clearness of the Martian atmosphere on different days. Photograph Fig. 2, taken April 20, 1937, shows much cloudiness. Figure 3 taken a month later, May 20, displays the highest blue transparency of his atmosphere ever observed, the clarity being evidenced by the showing of the dark surface markings. Figure 4 is a blue photograph of the same face of the planet, June 24, 1937, which illustrates how completely the sky of Mars has hazed over again. For on June 24 his air had again become opaque to blue light, and no trace of his surface markings shows through. These photographs not only prove the existence of a very considerable atmosphere about Mars, but also clearly show that he undergoes meteorological changes not unfamiliar to us here. Figures 5 and 6 are yellow and blue images of another face of Mars taken on May 13, 1937, which display the morning side of the planet covered with a blue-white haze, while the afternoon side was quite clear. Figures 7 and 8 are yellow and blue photographs taken June 1, 1937. Here the blue image shows new large dark areas which are as indicative of the influence of the air of Mars as are their associates, the bright clouds. That these strikingly dark areas on the blue photographs do not belong to the body of the planet is made evident by the yellow photograph, and confirmed by visual observations. It is difficult to say how his atmosphere causes these new "dark clouds"; whether they are due to strong local absorp-

tions or to polarization effects or to some other cause it is not now possible to decide.

Other photographs of Mars (not reproduced here) were presented that displayed the southern snow cap at its winter maximum 3,500 miles across in the one case and at the usual summer minimum of 150 miles in another. Another set of photographs taken in two different Martian summers displayed the peculiar disintegration of the south snow cap that has repeated itself at the same Martian date since 1845, pecularities which stamp the summer cap as a deposit on the planet's actual surface systematically dissolved with the march of summer there (Pl. IV). Still other photographs not only revealed the canals, but it was shown by comparison photographs that they also change greatly in inherent strength from time to time. A natural color photograph of Mars was shown which displayed the colors of his surface, portraying the "desert" regions reddish, the dark regions bluish-green, the morning and evening mist bluish-white, and the snow spots nearly pure white.

The surface temperature of Mars, the order of which has been indicated by the melting snow caps and other manifestations of his markings, was deduced years ago by Lowell from theoretical considerations as being about 48 degrees F. for the mean. More recently it was measured directly with the thermocouple by Coblentz and Lampland, and others, and the result is consonant with the earlier determination. The later measures show that around Martian noonday the temperature of the surface rises to 60 degrees F and higher. This temperature result is not surprising when we conisder his low albedo which shows that his surface reflects away only 15 per cent—*i.e.* absorbs 85 per cent of the solar energy reaching it—hence compensating largely for his greater distance from the sun.

Their wide variety and the significant changes his surface features undergo, stamp Mars as a planet the physical state of which is the most Earth-like of any of the planets. When to this we add his matchless canal markings which also

undergo significant change, we have the most engaging and at the same time one of the most complex planetary problems of the solar system.

Jupiter and Saturn

Jupiter and Saturn are observed to be so similar that we may well consider them jointly here. It is clear from these photographs that they differ widely from the terrestrial type of planets, Mercury, Venus, and Mars. The changeable and mobile character of their belted surfaces plainly reveal that they consist of cloud. But these clouds we know are not of water vapor as are our own, because their temperatures are too low, being about $-120°$ C. for Jupiter and $-150°$ C. for Saturn according to the radiometric measures. The composition of their cloud surfaces is not completely known. The spectrographic investigations by Slipher, Dunham and Wildt, and finally the complete identification by Slipher and Adel of all the important absorption bands in the spectra of these planets, have shown the overlying atmospheres contain ammonia gas and much methane. Their bright cloud belts may consist in some degree of crystals of frozen ammonia, but the dark and red belts and other colored darkish markings remain unexplained. The color-filter photographs, give evidence that the gaseous atmospheres of Jupiter and Saturn produce no appreciable scattering and are therefore very transparent; the absorption by the ammonia-methane atmosphere is confirmed by the darkening at the edges of the disk of the Jovian red photographs. The virtual absence of atmospheric scattering denotes that their superficial atmospheres are much less extensive than generally supposed, and suggests a molecular population of the order of the Earth's atmosphere or less. Thus with their cloud surface envelopes near the outskirts of their atmospheres, with the weak solar energy at their great distances, and without water vapor or other effective agent to bar radiation into space, their low cloud surface temperatures seem consonant with the other observations. But this low temperature does not necessarily

mean that their centers are cold. In fact the great turbulence of their atmospheric surfaces strongly evidences a considerable temperature gradient beneath. Hence, below their high floating cloud surfaces probably lies an atmospheric ocean which gradually grows denser and warmer downward, before it reaches any solid surface; indeed that these planets possess any solid surface even remotely resembling the Earth's crust is very doubtful.

Thus, it has become clearly evident that the surfaces we observe on Jupiter and Saturn are of clouds of some strange composition, being entirely different from the terrestrial type and probably composed of an unfamiliar compound. Also it is strongly indicated that the energy that gives rise to these changeable and varigated cloud surfaces comes from within the planets themselves, as there is no observed evidence that they are modified or influenced by diurnal, or seasonal effects of solar radiation.

Plate VII are photographs of Saturn, taken from 1909 to 1937, and show various aspects of this beautiful planet and its unique system of rings, and reveal the belts and other features which characterize his surface.

Plate VIII is a drawing and photograph of Saturn showing the great white spot which appeared on his equator in August, 1933. This proved to be the most remarkable cloud disturbance that has been observed on this planet since 1876, when a similar spot was observed by Hall at Washington. While we do not know certainly the cause of this phenomenon, we think it might have been due to ammonia vapor which rose from his interior and froze as it reached his high and cold cloud surface.

Plate IX are photographs of Jupiter which exhibit in Figs. 1 and 2 the varied types of features and great complexity of detail that mark his surface in periods of unusual activity. Figures 3 and 4 are two photographs of the same face of Jupiter taken two months and a half apart. They display the widespread changes which may occur there in a short time during periods of great activity. On the upper left side

of the disk is the Great Red Spot, a Jovian feature which in its general outline has been fairly stable and persistent for *two hundred and seventy years*. This permanency of the so-called Red Spot is in marked contrast to the general changeableness of his other markings.

Plate X displays examples of photographs of Jupiter taken at Flagstaff from 1909 to 1937, which are explained in the notes accompanying the plate.

By piecing together the various bits of evidence we find that the planets differ widely not only in their characteristic surface features but actually in physical conditions. In these respects the major planets stand in a class by themselves and are quite similar. But the terrestrial planets, each presents an individual case, and their surface conditions differ very widely. Like statements apply to their comparison as regards their atmospheres based upon spectrum and related observational data.

Recapitulating regarding the important matter of their atmospheres, the observations indicate that Mercury possesses no appreciable atmosphere; that Venus has a considerable atmosphere which is largely impenetrable to all wave-lengths of light and therefore its extent and composition is imperfectly known; that Mars has a considerable atmosphere but less than the Earth's and containing scantier amounts of water vapor and oxygen although displaying clouds and familiar meteorological effects, and that his atmosphere is generally transparent to yellow and red light but is peculiarly opaque to light of the violet end of the spectrum, although variable in this respect; and that the superficial atmospheres of Jupiter and Saturn are probably denser than that of any of the above planets and of a different composition containing large amounts of methane and some ammonia, but these are very transparent and manifest themselves chiefly by the greater selective absorption of the red rays.

From these studies it has become evident that, of all the planets, Mars most closely resembles the Earth. The temperature of his surface, the character and behavior of his

atmosphere, the presence of clouds, polar caps, and blue-green areas, and all the seasonal manifestations of his surface compose a picture which clearly stamps him as a body the physical state of which does not differ widely from that of our Earth.

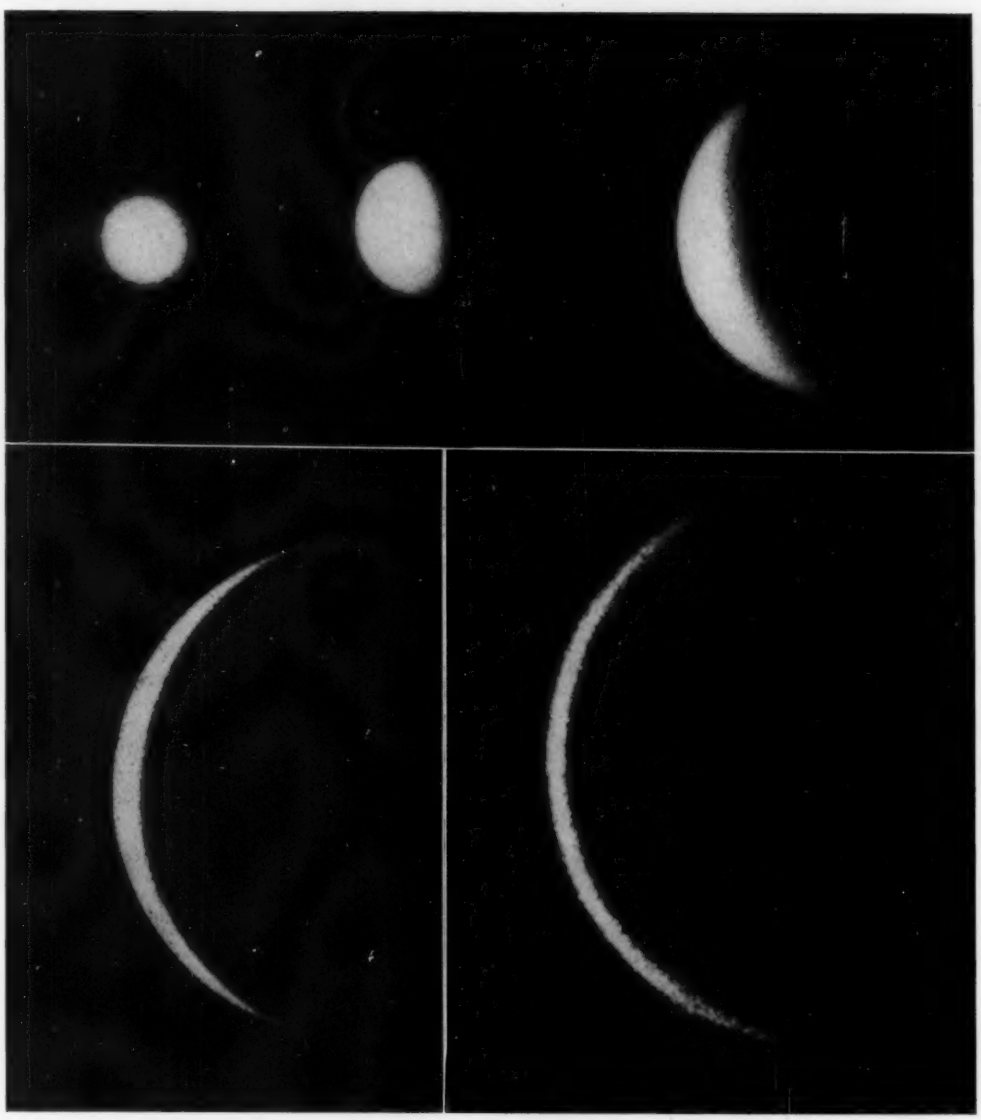
In this brief account of the Flagstaff observations of the planets possibly enough has been given to show that they include a wide field for study and that real progress has been made in the study of these companions of the Earth. Yet many questions remain pressing to be answered concerning their surfaces and atmospheres.

EXPLANATION OF PLATE I

VENUS

Photographs showing the various phases and true relative sizes of the planet's disk presented during a synodic period.

PLATE I



EXPLANATION OF PLATE II

Venus

Photographs taken in ultra-violet light at different times displaying ephemeral nature of the markings obtained by short-wave light.

In this plate are reproduced some representative examples of ultra-violet photographs of Venus such as have been taken at Flagstaff during the past ten years with the hope of gaining further knowledge of her rotation period. Photographs of Venus taken in different regions of the spectrum from λ 3500 to λ 12000 have failed as yet to record distinct markings, except those taken in ultra-violet light. This series of ultra-violet photographs were taken at different times and display the general appearance and behavior of the markings.

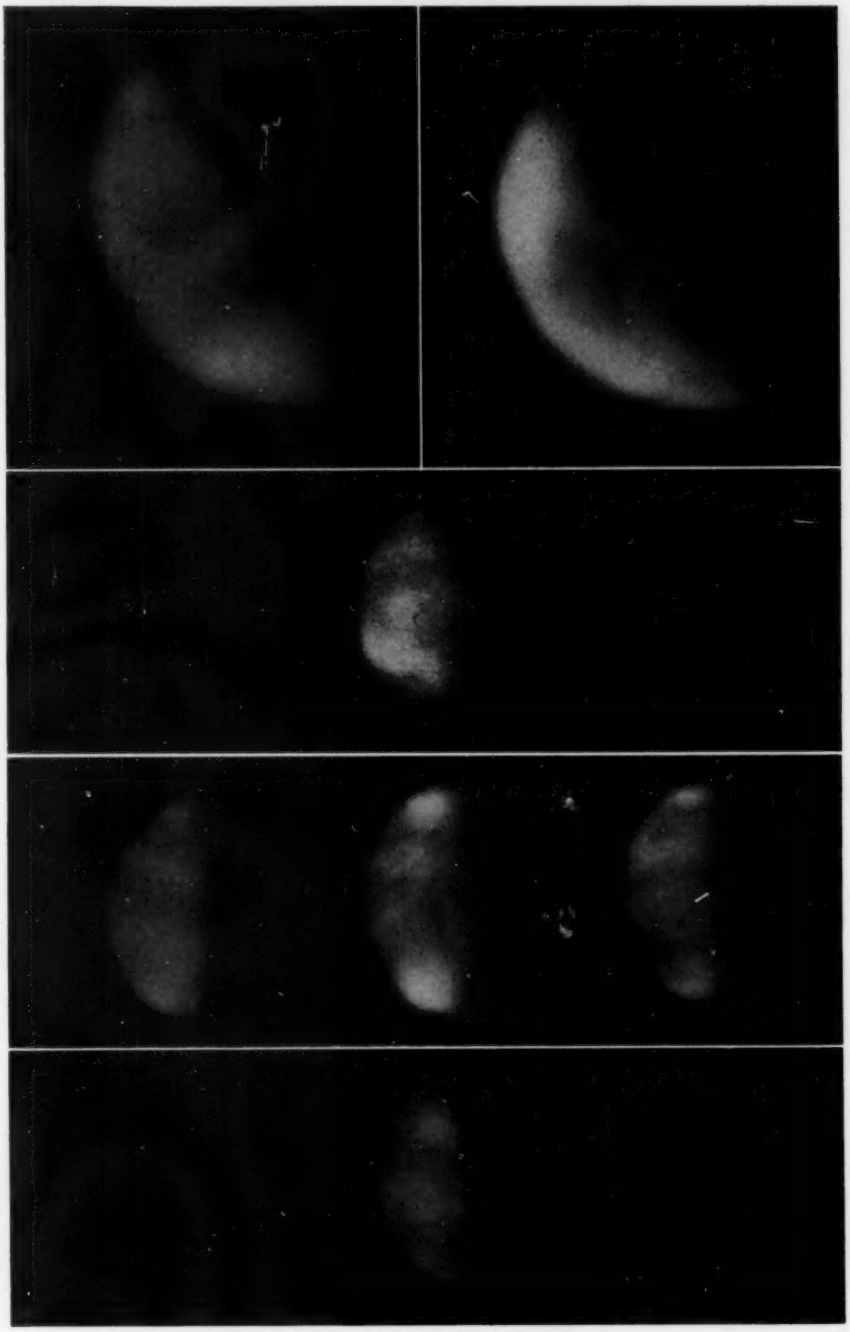
Fig. 1. An ultra-violet photograph taken February 2, 1937, when the planet was near half phase, which displays the surface strongly marked with bright areas at top and bottom and dark areas in between.

Fig. 2. This photograph was taken February 17, 1937, and represents about the minimum of markings, being only weakly marked and the disk appears quite uniform.

Figs. 3, 4, 5, 6, and 7. Here is a series of such ultraviolet photographs taken on three consecutive days, Fig. 3 on June 12, Figs. 4, 5, and 6 on the 13th, and Fig. 7 on the 14th, 1935, and therefore should be considered together. Those on June 13, were taken at 2:50, 5:50, and 7:15 P.M. It will be noted that the markings differ on the three days, and even those made at intervals of a few hours on the same day reveal differences in the bright hoods at top and bottom of the disk and in the other markings. But the differences disclosed are due to inherent change in the markings themselves and not to rotational shift.

Study of many thousands of such photographs made at Flagstaff in the last decade has shown nothing attributable to rotational shift, but on the contrary appears to evidence that the planet turns very slowly.

PLATE II



1

2

3

4

5

6

7

EXPLANATION OF PLATE III

Mars

These are actual photographs of Mars taken through a large telescope showing successive stages in the gradual decrease of the snow at the pole, and the darkening of the planet's tropics with the advance of Martian summer.

For the different photographs the Martian seasonal dates, corresponding to our calendar dates for the northern hemisphere of the earth, are:

No. 1, March 9. Here the south snow cap is mostly hidden in the antarctic night, but the latitude of the visible edge shows that the snow was approximately 3,300 miles in breadth.

No. 2, May 11. Here the cap is much reduced in size and the dark areas in the tropics are beginning to strengthen.

No. 3, May 29. Snow cap is much further reduced in size and the central dark areas have correspondingly increased in intensity.

No. 4, June 23. The snow cap here is but a few degrees across, and the dark blue-green band is very strong.

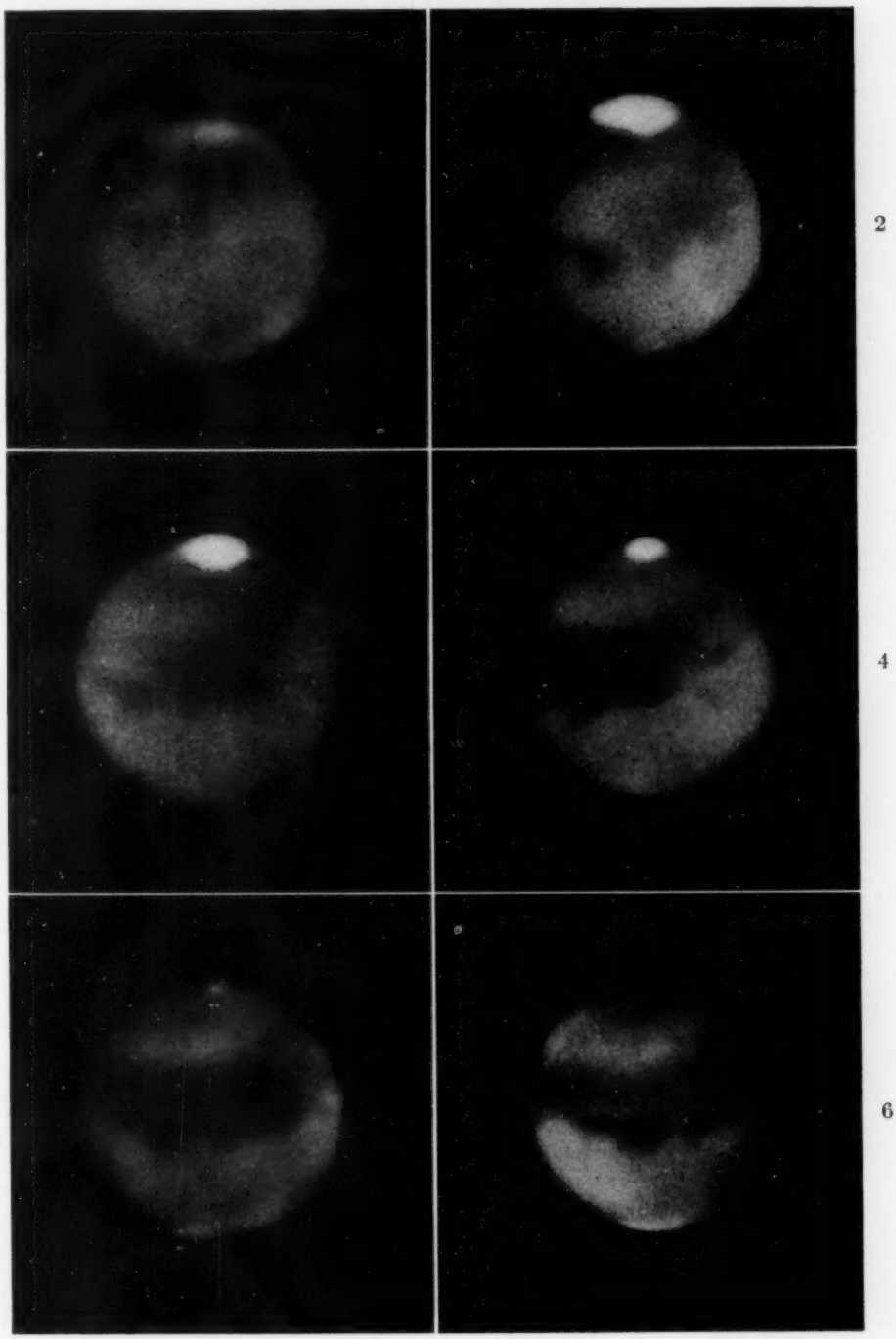
No. 5, July 31. The polar cap is small, and the tropical band has developed its maximum intensity.

No. 6, August 21. The polar snow is nearing its normal minimum, being only about 150 miles in diameter. The tropical dark band is still intense but a little less so than it was at the end of Martian July.

Note that as Martian summer advanced, and the tropics darkened, the polar regions and higher latitude grew a little lighter in tone than they were in Martian spring.

This gradual darkening of certain regions of the planet in his summer season and their subsequent fading in winter seems best explained by assuming that these dark areas are due to vegetation.

PLATE III



EXPLANATION OF PLATE IV

MARS AND ITS CANALS

Sept. 7, 1924
Sept. 11, 1924
(Trumpler)

Sept. 10, 1924
Sept. 9, 1924
(Slipher)

Drawings of Mars by two observers working independently at two different observatories. The drawings at the left are by Dr. Trumpler made with the great 36-in. refractor of the Lick Observatory, and those at the right are by the author with the 24-in. refractor of the Lowell Observatory. The observations agree in all essentials and confirm the existence of the canals.

The detached spot of snow seen to the left of, and below, the main cap is known as the Mountains of Mitchell, discovered in 1845, and marks a definite milestone in the summer melting of the cap, having been observed there on the same date each Martian summer favorable for its detection since its discovery.

PLATE IV

EXPLANATION OF PLATE V

Great Storm on Mars

Photographs displaying the life history of an unparalleled storm over the Martian tropics (bright area near center of disk) on July 9–12, 1922. Area about 400,000 square miles.

Both the visual and photographic observations so clearly revealed the atmospheric nature of this bright area as to leave no doubt that it was Martian clouds passing over the surface of the planet. The row of images at the left side are drawings from independent visual observations, and at the right are photographs made on corresponding dates as follows:

Fig. 1. Drawing July 8, displaying the normal aspect of Mars the night before the storm.

Fig. 2. Photograph June 6, showing nothing unusual present there on that date.

Fig. 3. Drawing July 9, displaying the storm in the form of a large bright spot which blotted out the true surface of the planet.

Fig. 4. Photograph July 9, showing the storm area on the night of its first appearance. It will be noted that the cloud area is nearly as bright as are the polar caps.

Fig. 5. Photograph July 9, taken through a blue color-filter with the 42-inch reflector by Dr. Lampland. This violet light photograph does not show the cloud area any brighter than the rest of the planet's disk, because of the strong yellowish tint of such Martian clouds and because of the Martian atmosphere correspondingly reflected so much blue light as to level out the whole disk of the planet. Note how much larger and brighter the polar caps appear in the blue than in the yellow filter photograph.

Fig. 6. Drawing July 10, displaying marked change in the form of the storm area and revealing that it was drifting downward toward the north pole.

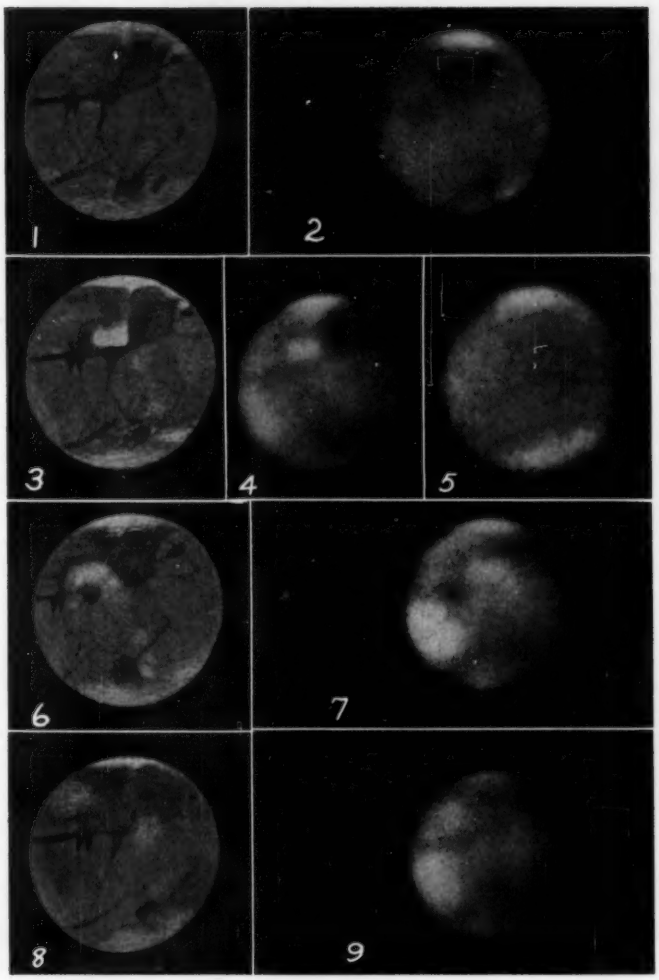
Fig. 7. Photograph July 10 confirming the change depicted in the drawing of this date.

Fig. 8. Drawing July 12, showing feebly the remnant of the storm to the right of and below where it first appeared.

Fig. 9. Photograph July 12, displaying the last vestige of the subsiding storm.

This is the most remarkable midday storm ever recorded on Mars. By its movement over the Martian surface and its general behavior it revealed its cloud-like nature, and gave objective proof of the existence of an atmosphere about Mars.

PLATE V



EXPLANATION OF PLATE VI

MARS

Direct photographs of the planet illustrating various degrees of clearness, of cloudiness, and of general opaqueness of the Martian sky in 1937.

These photographs are intended to illustrate various atmospheric phenomena on Mars, such as the usual great opacity of his air to blue and violet light, and examples of blue white and of "dark clouds." Such atmospheric phenomena on a planet are best revealed by comparing photographs secured through red or yellow light filters which cut out the atmospheric haze, and photographs through blue or violet filters which obviously will register the maximum effect of a planet's atmosphere. The photographs below reveal some interesting effects of the Martian air.

FIGS. 1, 2, 3, and 4. This group of photographs portray one of the most remarkable cases of clearing up of the *Martian air* ever observed. All four photographs were taken in 1937 and are of the same face of Mars as that shown by the red light photograph in Fig. 1, made on May 20. Figure 2 is a blue light photograph taken on April 20 when the Martian air was so opaque to the short wave-lengths that not enough got through to reveal any trace of the true surface markings. However, in the blue photograph, Fig. 3, taken May 20 his air was so transparent to these same wave-lengths of light as to reveal the actual surface markings with amazing distinctness. Later, however, in June the Martian air again became opaque and no trace of the actual body of Mars is recorded in the blue light photograph in Fig. 4, taken on June 24.

This example clearly demonstrates that the great difference between red and blue photos of Mars is produced by his atmosphere, and further evidences how great is the scattering power of the Martian air at one time and how small it may be at another. Comparing the atmospheric effects in such photographs of Mars with terrestial landscape photographed likewise through different path lengths of our atmosphere at Flagstaff leads to the conclusion that the opacity of the Martian air is generally about twice that of our own atmosphere.

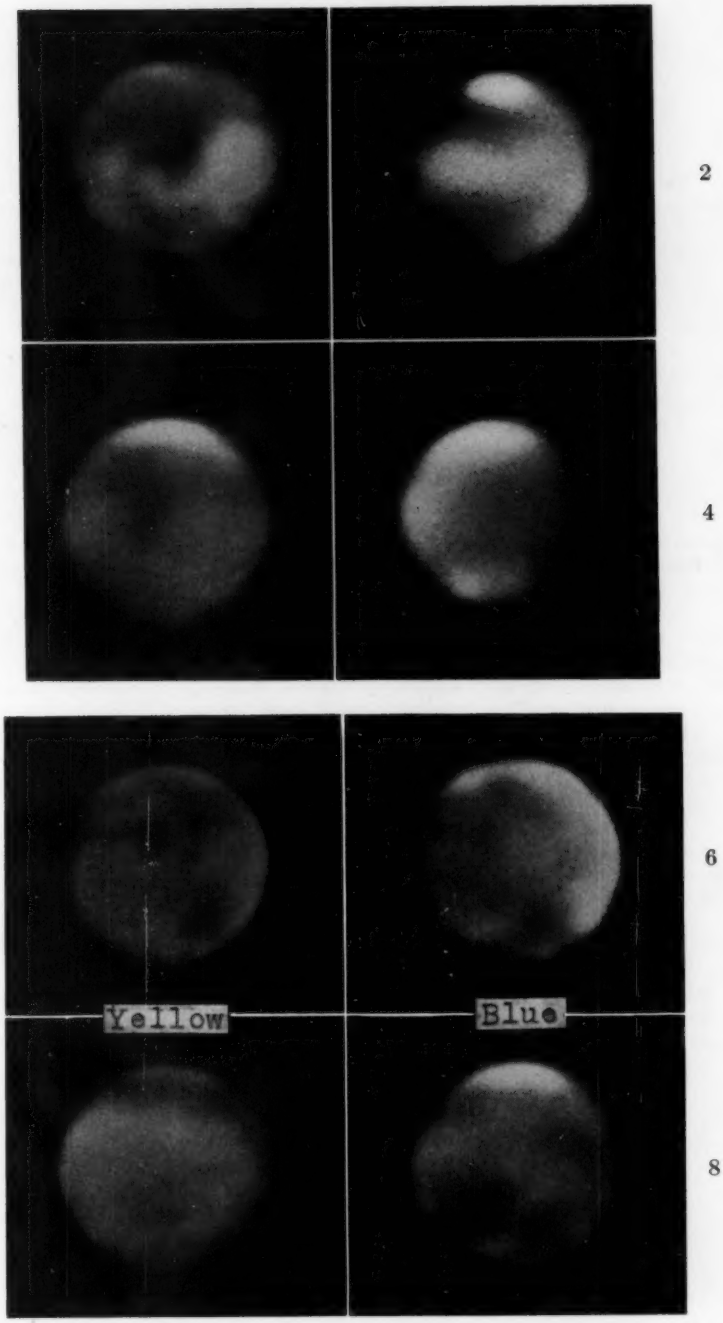
FIGS. 5 and 6. Here are shown two photographs of the planet made on May 13, 1937, the one at the left through a yellow color-filter and the one at the right with blue light. The blue photograph denotes by the extensive white areas over the right-hand portion of the disk that the forenoon sky of Mars was extremely hazy, while the showing through of his actual surface features toward the left side betokens that his afternoon sky was unusually clear.

FIGS. 7 and 8. This pair of photographs (Fig. 7 in yellow and Fig. 8 in blue light) taken on June 1, 1937, display the amazing effect of the Martian air in producing "dark clouds." It will be seen that the large dark areas revealed by the blue photograph do not belong to the body of the planet as the yellow image discloses, but were caused by the planet's atmosphere. Photographs made on other dates before and after this one revealed the temporary character of these dark areas and indicated their atmospheric origin.

Mars 1937

May 20, Red
May 20, Blue
April 20, Blue
June 24, Blue

PLATE VI



EXPLANATION OF PLATE VII

SATURN

From 1909 to 1937

Representative photographs displaying various aspects of the ball and ring system.

These views are a few examples from the photographic record of Saturn secured at the Lowell Observatory during the last 28 years; they show the planet as seen in a powerful telescope magnifying about 1,000 times, thus each image embraces an area approximately 125 times that of the Moon as seen with the naked eye.

It will be noticed that the planet displays parallel belts resembling those of Jupiter. These belts, like their counterpart on Jupiter, are variable as to position, form, and intensity as may be seen in the photographs.

Fig. 1. Photograph taken November 4, 1909, displaying the upper hemisphere as quite uniformly dark and nearly beltless. Note the shadows of the ball upon the rings right side, and of the rings on the ball below.

FIG. 2. Photograph made February 11, 1916, which shows the upper hemisphere not as dark as in 1909, but distinctly belted. It will be seen that the ball shines through Cassini's division and the outer ring evidencing how sparse the ring particles are. Note the dark sash across the ball along the upper edge of the rings, which is due to its inner Crepe ring—seen against the ball.

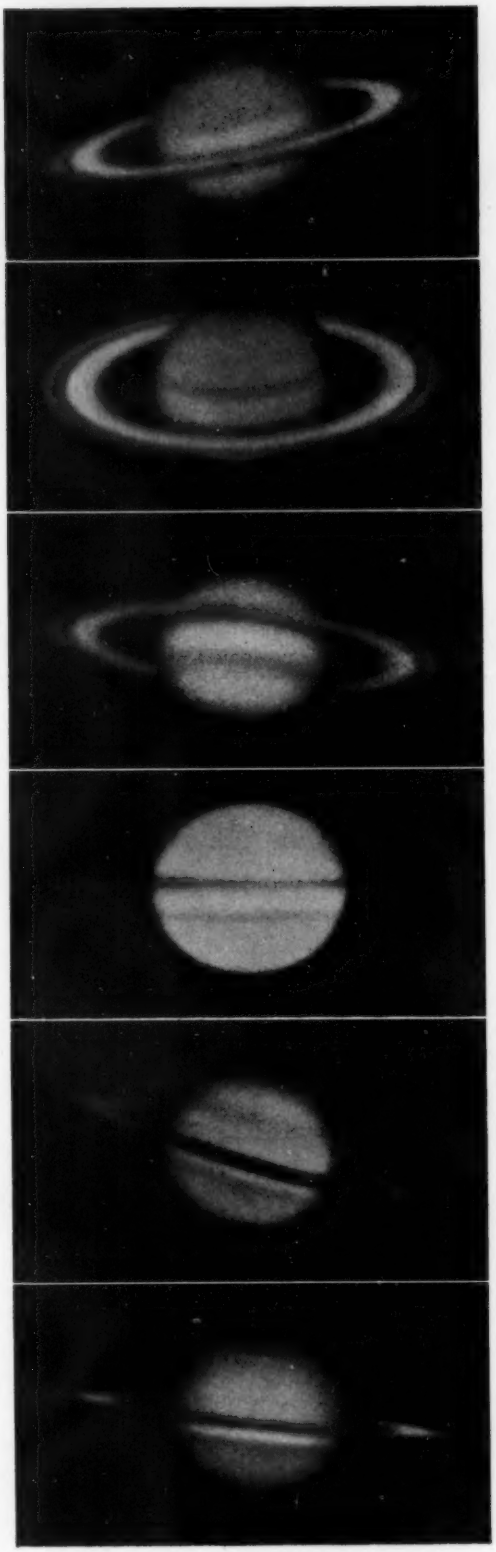
FIG. 3. Photograph September 18, 1934, displays the northern hemisphere of the planet and the northern face of the rings. Especially noteworthy is the strength of the dark north tropical belt here, it being the most prominent dark belt observed on Saturn since 1904, and closely resembles the strong tropical belts common to Jupiter.

FIG. 4. In this photograph made July 29, 1936, we see the ring system turned edgewise to us. In this position the rings become extremely narrow and faint, so faint that they show only feebly in a normal photograph. Against the ball of Saturn the projected rings and their shadow appear quite dark.

FIGS. 5 and 6. The excessive faintness of the rings in Fig. 5, July 16, 1937, was due to the Earth being higher above the ring plane than was the sun; thus allowing us to see effect of one particle of the ring system shadowing the next behind it and so on, so as to cause a fainter surface of the rings than would otherwise obtain for the same tilt of the ring system. Whereas in Fig. 6, October 1, 1937, the reverse is true, the sun is higher than the Earth above the rings, and all the visible surface is fully illuminated and the rings are far brighter. This offers visible evidence again of the already known meteoric constitution of his rings which was proved mathematically by Maxwell and later spectroscopically revealed by Keeler.

(Photograph number one by Dr. Percival Lowell, the others by the author.)

PLATE VII

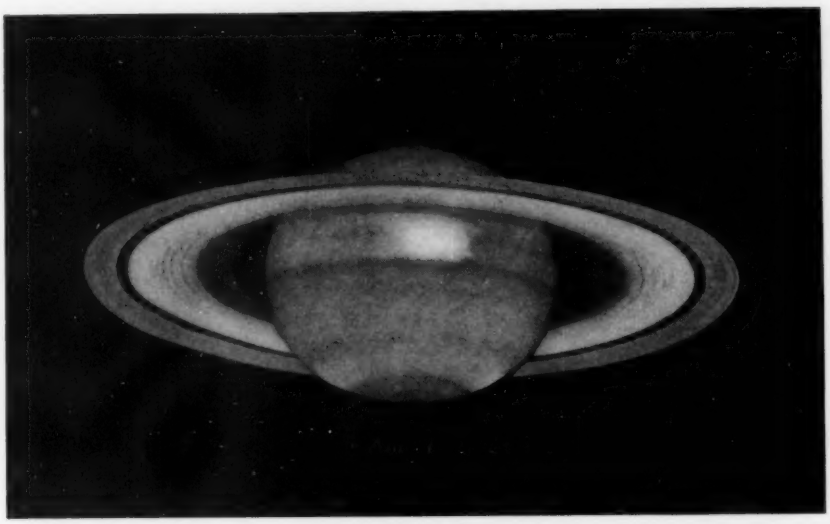


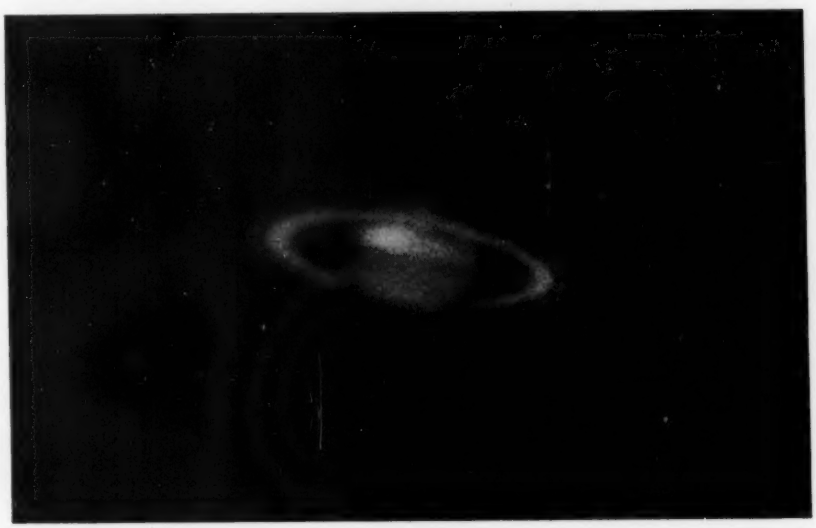
EXPLANATION OF PLATE VIII

The Great White Spot of Saturn in 1933.
Drawing August 7, 1933.
Photograph August 9, 1933.

This outburst proved to be the most phenomenal feature recorded on Saturn in more than fifty years. The spot appeared very suddenly between August 2 and 3, 1933, as an exceedingly brilliant white spot of elliptical form and about 12,000 miles long, subsequently developing in a forward direction (to the left) until by September 11 it was fully 50,000 miles long, after which it faded to invisibility by early October. Meanwhile some eighteen other similar white spots, but smaller and more temporary, appeared in different longitudes in line around the planet only to subside even more quickly than did this major spot. Timing the transits of the main spot and one other during 211 revolutions of the planet gave a rotation period of 10h 14m 24.2s for Saturn, agreeing closely with the period of 10h 14m 23.8s derived by Asaph Hall from a similar great white spot in December, 1876.

PLATE VIII





EXPLANATION OF PLATE IX

JUPITER

September 23, 1928 October 6, 1928

The two photographs at the top illustrate the wide variety, and great complexity of detail over his surface other than the usual parallel belts.

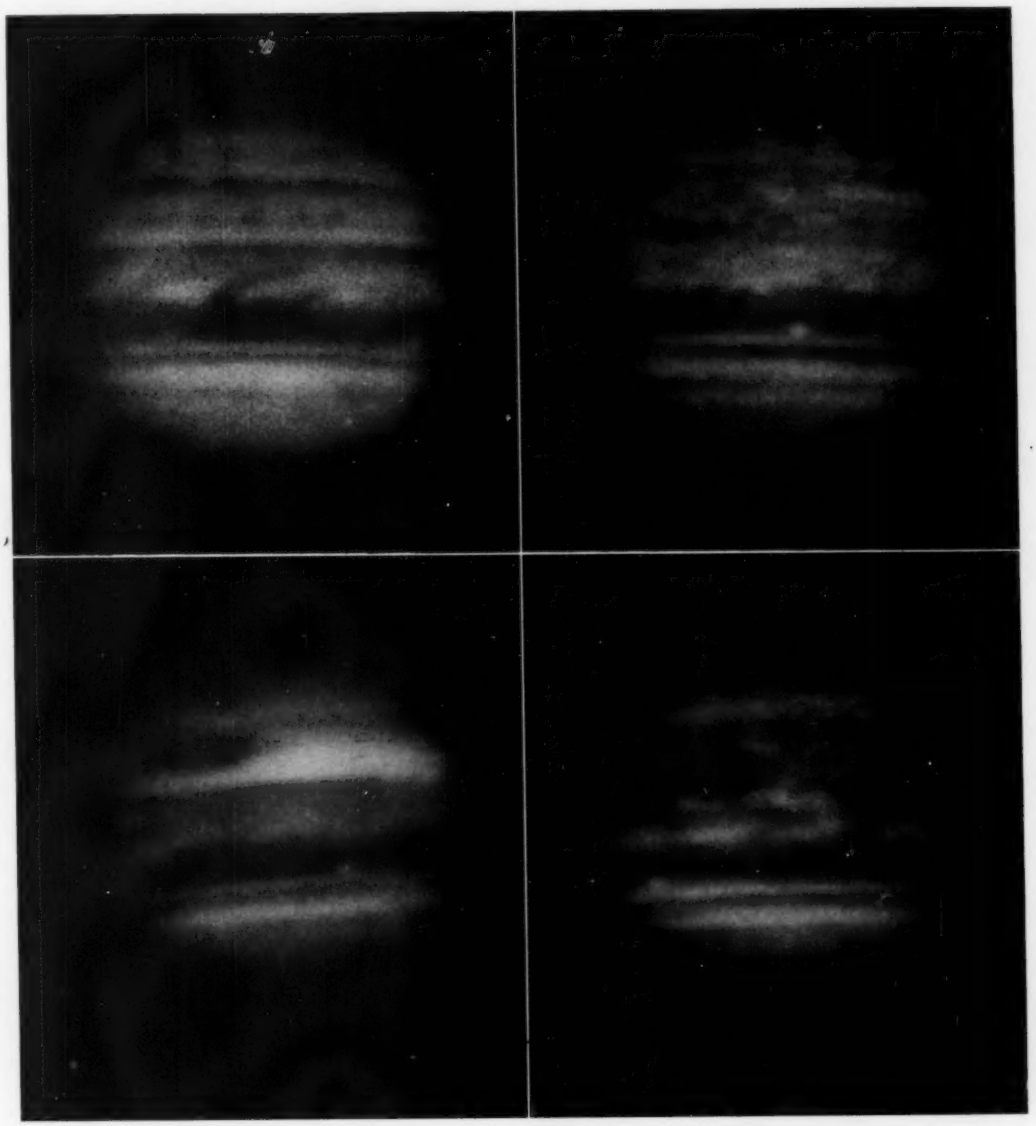
September 14, 1928 November 30, 1928

The two photographs at the bottom illustrate the rapid changes which sometimes sweep over the planet. These two photographs are of the same face of the planet taken two and one-half months apart and display the great transformation of the equatorial and south tropical zones. The great bright zone displayed by the left-hand photograph has become darkened in the second example by clouds of some dark material which has invaded much of the equatorial zone also. The Great Red spot, the dark oval seen near the left side of the first photograph is inconspicuous amidst its dark surroundings in the second photograph. The mobile character of his markings is manifested by the way spots and belts in one latitude flow past those in another latitude, those nearest the equator generally moving more rapidly than the spots in higher latitudes.

Jupiter, 1928

September 23	October 6
September 14	November 30

PLATE IX



EXPLANATION OF PLATE X

JUPITER (1909–1937)

In these few examples of photographs of Jupiter, which are representative of the more than one-hundred thousand secured at Flagstaff during the last thirty years, it is intended to illustrate how the planet looked at different times, reveal some more or less typical changes, and display some interesting Jovian phenomena.

FIG. 1. A photograph of Jupiter with a yellow color filter taken April 27, 1909, which displays the intricate structure of the surface and at the same time evidences something of the quality of the early photographs.

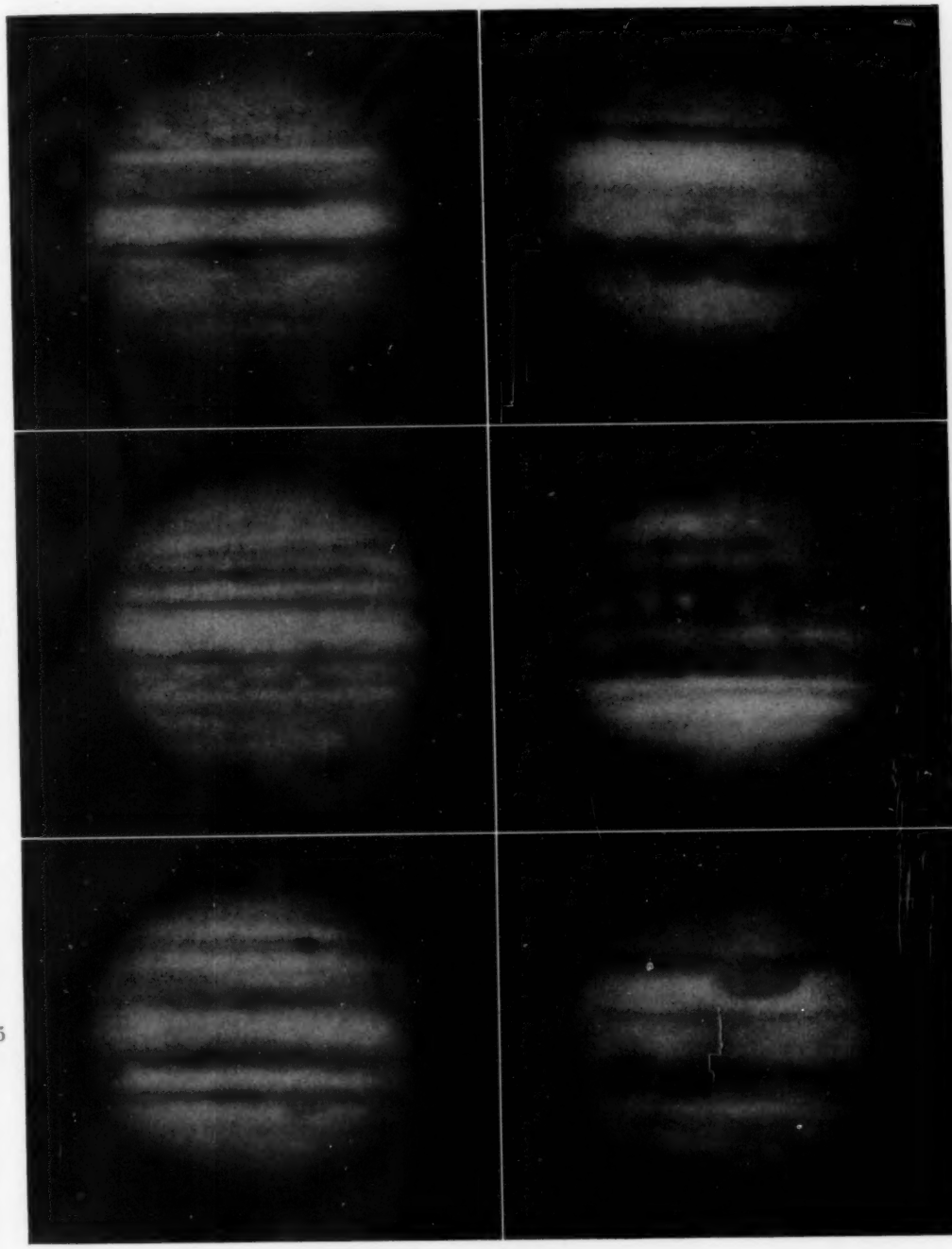
FIG. 2. Photographs taken in the same light as Fig. 1 on July 29, 1936, portraying a marked alteration in the belts.

FIG. 3. A photograph made September 23, 1914, which, besides displaying in great detail the precise appearance of the planet with a queer row of port-hole markings along the dark north tropical belt, records a total solar eclipse in progress there. The eclipse shadow of satellite III is conspicuous to the right of the center of the disk, while, the image of the satellite itself may be faintly discerned against the sky background at the extreme left of the cut.

FIG. 4. This photograph made November 23, 1928, displays the planet when in a highly disturbed state as shown by the intense bright and dark markings which give a broken and disrupted appearance to the south tropical zone. It is obvious here that the darkest spots are of very low albedo as compared to the brighter regions which suggests the wide diversity in the state of the material composing the visible surface.

FIGS. 5 and 6. These photographs taken in yellow light on February 11, 1930, and on June 10, 1936, respectively, show the oval-shaped Great Red Spot a little above and to the right of the center of the disk in both cases. These comparable photographs illustrate the remarkable changes this spot, the most permanent marking on the planet, undergoes from time to time. In the first example the Red Spot was very white and is here shown as the brightest part of the disk, while in the second case it was dark red and stands out as one of the darkest parts of the disk.

PLATE X



EXPLANATION OF PLATE X A

FIGS. 7, 8, and 9. This set of three photographs was taken within a few minutes of one another on June 14, 1937, in yellow, deep red, and blue light. They reveal the transparent nature of the Jovian atmosphere above the belted surface, and the dominant influence of color in modifying the appearance of the planet in photographs from light of widely separated regions of the spectrum. The great transparency of his atmosphere above the belts is denoted by the belts showing clear to the edge of the blue photograph. The effect of color is disclosed by the difference between the red and blue photograph especially noticeable in the Red Spot where the intense redness causes it to turn dark by blue light, but nearly as bright as the rest of the disk in red light. Thus, such color-filter photographs, taken practically synchromously through different color filters, exhibit rather large differences in the appearance of the markings according to the filter used because of the color reactions of the vari-colored markings themselves (i.e. reds photographing as bright through the red filter and as black through the blue, etc.) and not by any obscuring influence of the overlying atmosphere, as some investigators have thought to be the case.

JUPITER

7 8 9

PLATE X A

RECENT STUDIES IN SOLAR PHENOMENA

ROBERT R. McMATH

Director, McMath-Hulbert Observatory, University of Michigan

(Communicated by Heber D. Curtis)

(*Read, April 22, 1938*)

ABSTRACT

This paper was delivered at the Annual Meeting of the Society on April 22, 1938, and was based upon two reels of motion pictures illustrating the points treated. While given by the writer, the paper in a very real sense may be regarded rather as a report of the research work of the McMath-Hulbert Observatory of the University of Michigan at Lake Angelus, Michigan, in the new field of solar studies by the motion picture method, and in which several have collaborated; Director R. R. McMath, our Research Associate, Dr. Edison Pettit of Mount Wilson Observatory, Assistant Astronomer, Harold E. Sawyer, and Assistant, John Brodie of the McMath-Hulbert Observatory staff, as well as one or two others who have assisted for shorter periods. To all of these colleagues, the writer desires to extend his grateful appreciation, and also to Dr. Heber D. Curtis, Director of the Observatories of the University of Michigan, who has given constant encouragement and every support within his power to this line of investigation since its very inception six years ago.

The difference in the requirements and in the treatment between an oral presentation and the needs of the printed page is, furthermore, so wide that it has seemed preferable to recast the oral presentation completely in publication by adding data impossible to treat with any completeness while the films were being shown.

THE ASTRONOMICAL MOTION PICTURE

As applied to the scientific objective of securing a continuous record of celestial phenomena showing motion and change, the instrumentation for an astronomical motion picture would, at first sight, appear to involve merely the substitution of a motion picture camera for the photographic plate ordinarily used with a telescope. In actuality, the instrumental details and manipulation are highly complex and exacting, and the successful motion pictures of solar prominences now being secured at Lake Angelus are in a very real

sense the result of a gradual process of experiment and evolution extending over the past twelve years.[1]

While the technique was originally developed for motion pictures showing such phenomena as sunrise and sunset on the moon, the rotation of planets on their axes and the revolution of satellites about them, etc., the illustrations we are about to consider are drawn from the subsequent application of this development to solar phenomena. In general, as applied to the sun, exposures by the motion picture method are quite short, from the standpoint of the astronomer, and anywhere from a few hundred to over a thousand such separate exposures or *frames* will be taken in the course of a suc-

[1] Earlier papers on the motion picture method and results may be found in the following media:

Some New Methods in Astronomical Photography, with Application to Moving Pictures of Celestial Objects. By Francis C. McMath, Henry S. Hulbert, and Robert R. McMath, *Publications of the University of Michigan Observatory*, **4**, 55–73, plus 11 Plates (1931).

The Spectroheliokinematograph. By Robert R. McMath and Robert M. Petrie, *ibid.*, **5**, 103–117, plus 9 Plates (1933).

A New Method of Driving Equatorial Telescopes. By Robert R. McMath and Walter A. Grieg, *ibid.*, **5**, 123–131, plus 2 Plates and 4 Diagrams (1934).

A Short-Lived Solar Disturbance. By Robert M. Petrie and Robert R. McMath, *ibid.*, **6**, 43–44, plus 1 Plate (1934).

Relative Lunar Heights and Topography. By Robert R. McMath, Robert M. Petrie, and Harold E. Sawyer, *ibid.*, **6**, 67–76, plus 8 Diagrams (1935).

A full description of the new tower telescope and its mechanism is given in—The Tower Telescope of the McMath-Hulbert Observatory. By Robert R. McMath, *Publications of the University of Michigan Observatory*, **7**, 1–56, plus 20 Plates and 8 Diagrams (1937).

See also: Motions in the Loops of Prominences of the Sunspot Type, Class III*b*. By Robert R. McMath and Edison Pettit, *Publications Astronomical Society of the Pacific*, February, 1938.

Some New Prominence Phenomena. By Robert R. McMath and Edison Pettit, *ibid.*, October, 1937.

A Prominence of Record Height and Velocity. By Robert R. McMath, Edison Pettit, Harold E. Sawyer and John Brodie, *ibid.*, December, 1937.

Prominences of the Active and Sunspot Types Compared. By Robert R. McMath and Edison Pettit, *Astrophysical Journal*, **85**, 279–303, plus 8 Plates and 6 Diagrams (1937). Mount Wilson Contributions, No. 568.

The Effect of the Projection Factor on the Motions in the Loops of Class III*b* Prominences. By Harold E. Sawyer and John Brodie, *Publications of the University of Michigan Observatory*, **7**, 79 (1938).

Prominence Studies. By Robert R. McMath and Edison Pettit, *Astrophysical Journal*, **88**, (1938).

Characteristic Features of Solar Prominences. By Edison Pettit, Mount Wilson Contributions No. 451, *Astrophysical Journal*, **76**, 9, (1932).

cessful day's run on a prominence. It will be manifest that adjustments for a number of variables must be provided that are very different from those attending the ordinary exposure on a celestial object, and some of these will now be enumerated.

General Considerations

1. There must be a high degree of uniformity in the successive frames of a run, and thus all the processes of development, fixing, washing and drying must be carried out under perfectly controlled conditions. The commercial film laboratory has developed all such photographic processes to a high degree of perfection, employing baths of uniform chemical constitution, closely controlled as to temperature, with humidification or de-humidification of the surrounding air during the drying processes as may be necessary, and with the employment of air washing or air filtering methods to guard against dust. It is impossible for any ordinary dark room installation to provide such uniformity of processing except at great cost. Accordingly, the standard 35 mm film we use is sent to some commercial motion picture photographic laboratory for processing. The well-equipped small dark room of the McMath-Hulbert Observatory is ordinarily used only for the development of the routine scout or check photographs that are made at regular intervals, to keep track of any developments in a prominence under investigation which may necessitate readjustment of the apparatus.

2. It must be possible to provide for any desired length of exposure and for a control of the "dark time" between the exposures on successive frames. In the solar work, the dark time is sometimes reduced to as little as three seconds, but in work on the moon or planets it is frequently necessary to provide for longer intervals between exposures, to secure some desired pictorial effect, or a certain duration for the run when projected on the screen. Not only must the timing be automatic so that the observer need give no further attention to the matter after he has once selected the proper exposure and dark-time, but he must be able to alter this exposure-time

during a run. As the sun sinks down toward the West, its rays pass through a greater thickness of air, for example, necessitating an increase in exposure-time; such an increase may also be found necessary because of a falling off of transparency due to haze or thin clouds at any time.

A motor driven gear-train in an underground control room automatically makes the exposures in the special motion picture camera, requiring no further attention from the observer after the relative lengths of exposure time and dark-time have been settled upon, and the observer, from his working position, may change the *exposure* time at will.

3. We are not in the business of making "movies" that may delight the student or the astronomer by the grandeur of the motions and changes in the ever-changing maelstrom of a prominence in action. Were that our only aim, our films would have comparatively little concomitant scientific value. To possess a definite value as scientific records available for measurement, accurate and continuous time records must be kept, and this again with a minimum of attention on the part of the observer. Accordingly, by electrical means, the opening and closing of the motion picture camera-shutter is automatically recorded on a continuously running chronograph in the underground control room. With this chronograph-record and a frame-counter, the time may be read for any frame that is being measured for the study of a given prominence.

4. It is manifest that the registration of the individual frames of a scene must be as perfect as possible, to avoid flickering and lack of sharpness on the screen when the run is projected; this requires that the mechanism driving the mirrors of the tower telescope which in turn keep a beam of sunlight directed upon the optical parts of the photographic equipment, must function with extreme perfection. As a matter of fact, it was very early discovered that there should be no "guiding" whatever, in the ordinary astronomical sense of that term,—that is to say—there must be few, if any, adjustments of the position of the solar image during or be-

tween the frames of a run. This very difficult requirement made it necessary to secure a driving rate of high accuracy, which should, however, be also perfectly flexible and changeable at will by very minute amounts to compensate for the fact that during a run of several hours' duration there will be very small changes in the apparent rate at which the sun moves in the sky, due to the changing amount of the refraction suffered by the rays in passing through our atmosphere. It was thus found necessary to abandon the conventional concept of a telescope as an instrument to be guided through minute and relatively frequent slow-motion adjustments that necessitate continuous and eye-straining attention on the part of the observer, and to aim instead for an instrument that would be a *following* device, rather than one depending upon the human element of *guiding*.

This proved to be an exceedingly difficult problem, and has involved the trial of five different methods during the past eight years. It was very early found that the commercially controlled frequency of our source of input AC electrical current, the Detroit Edison Company, suffered fluctuations due to the heavy industrial load of the Detroit area that made it inadequate for our requirements. The details of our successive attempts to secure a suitable and sufficiently flexible driving rate cannot be given in this paper, but are described in the paper on the new tower telescope given in the reference literature cited above. Suffice it to say, that the solution was finally found in a controlled, or rather manufactured frequency of our own, secured through the use of resistance ballasted radio tubes. By this method, the input frequency can be accurately controlled, and with it the rate of drive of the telescope mechanism, within about 0.002 of a cycle cumulative error. It can also be instantly varied in steps of the same small fraction of a cycle through a push button control, giving a rate of drive that is for all practical purposes continuously variable. This form of telescope drive (*the McMath-Hulbert Electric Drive*) has functioned so perfectly that we feel that our problem has been solved; it has been used also in the new

McDonald Reflector in Texas, for telescope drives at The Lick Observatory, and is now under consideration for a number of large reflector mountings. The instruments at Lake Angelus were also the first to employ a similar delicately variable drive in the declination coordinate, in addition to the right ascension drive noted above. Of more particular value for a rapidly moving object like the moon, this drive in declination is also found necessary and is continuously used for all the solar work.

5. The astronomical motion picture shows significant differences from the conventional astronomical negative as regards both "publication" and measurement. It is, perhaps, a serious detriment that adequate publication is possible only through a projection of the film on a suitable screen. Individual frames, or frames at certain intervals, can, of course, be published, and will illustrate some of the observed phenomena of motion and change. Yet these are, at best, merely "stills," to employ the argot of the motion picture lot, and cannot compare with the impression to be secured by projection. It may well be said that, since this new technique of solar research shows us changes *as they are taking place*, it differs from all earlier work in that it offers for our study *changings* rather than *changes*, and that it gives us an intensely kinetic rather than a static picture of an event; it emphasizes the processes and gives us the results of the processes as well.

The complexity of the phenomena shown in a prominence in action all too often baffles description; the multitude of delicate features in motion is such that no succession of ordinary still pictures would permit their complete analysis; only when projected on the screen can the actual process become completely apparent. Very frequently faint details in a complex prominence will thus become apparent and clear only after repeated projections, though unnoticed in earlier reviews and, of course, they are entirely lost in an ordinary still picture. The tenth or twelfth showing may bring in numerous features undetected in the first few projections.

6. In another important aspect the motion picture differs from the single astronomical negative, merely because the former is kinetic and the latter is static. The compression of the ordinary picture in the space-factor, is, of course, self-evident, and obtains also in the film, since the actual dimensions of a frame in our usual focal length employed on the sun is about 170,000 × 320,000 km. However, we have an additional compression in the time-factor which may well be mentioned at this point. Our current exposures on solar features range from ten to thirty seconds, and thus a run that may have taken several hours to photograph is projected on the screen in perhaps not more than three minutes. To give a concrete illustration—assume that the exposure on each frame was twenty-seven seconds, with a uniform interval of three seconds of dark time between successive frames. Then two pictures will have been made per minute, to be projected later at the standard rate of sixteen per second; in this case the time compression factor will be 1 : 480. In current work, this compression factor will ordinarily range between 1 : 400 and 1 : 1200, depending upon the exposure time and dark interval used.

So far from being a detriment, this compression in the time factor is not only inevitable but a positive advantage. To illustrate this point, let us assume that a luminous knot forms 150,000 km above the solar surface, and then descends to the sun at the uniform rate of 60 km/sec, thus having rather average values both as to distance traversed and speed along the trajectory. The time of descent to the solar surface will then be about forty-two minutes, and this will also be the time necessary to record the motion on the film. Manifestly, we do not wish to wait so long to see this motion displayed on the screen; if the 84 frames at two per minute necessary to record this descent are projected at the standard rate, we shall compress the action into five seconds, a fairly convenient duration for observing and studying the descent of the luminous knot when we recall that several such knots will form in every scene.

7. The technique of measurement is likewise radically different from that of the ordinary celestial photograph. A long run on a clear day may last from 8:00 A.M. to 6:00 P.M., or later, and comprise over a thousand separate frames. This is rather an embarrassment of riches, from the standpoint of measurement, even though it will ordinarily be necessary to measure only the frames in certain sections of the film which record the special feature of prominence activity under examination. In the study of the active prominence of September 26, 1937, for example, more than five hundred separate photographs were measured for the motions of parts of the prominence.

But, as may be imagined, the measurement of a total of even one-tenth of the frames on a long run means a considerable expenditure of time and calculation. For this work of measurement, we have found the projection method the most economical in time and labor. The image of the film frame is enlarged to about 20 × 25 cm and projected on the reverse side of an opal glass screen in a special measuring engine designed by the writer.[1] Even greater magnification has been used in much of our work.

The Tower Telescope and Instrumentation

The new tower telescope of the McMath-Hulbert Observatory is the seventh installation of this type in existence. Inasmuch as it is the most recent of such instruments, past experience has been freely drawn upon in its design, for which the writer is responsible, and it has been possible thus to discard features of earlier designs that proved unnecessary or disadvantageous for this specialized research. It involves, also, many new improvements and refinements of design, and a schematic description at this point may serve to elucidate certain features of the solar results that have been secured, to be noted later.

The tower is fifty feet (15.2 m) in height, and stands over a steel-lined well thirty-two feet (10 m) deep. The tower

[1] Described and illustrated in the paper by Sawyer and Brodie, *loc. cit.*

structure is of very rigid design, composed of two concentric cylindrical steel shells with structural elements and foundations entirely separate; the outer tower carries the dome, stairways, etc. and protects the inner tower from wind vibrations; the inner tower carries the cœlostat and all optical parts of the instrument that are above ground. Under the dome at the top of the tower are located the mechanisms of the cœlostat, of very massive construction (7.5 tons). The light from the sun falls first upon a flat mirror 22 inches (56 cm) in diameter, from which it is reflected to a second flat 18 inches (45.4 cm) in diameter, which sends the beam vertically down the interior of the tower.

Above the spectroheliograph head in the work room at the base of the tower are several optical elements which are almost instantly interchangeable to obtain any one of five separate focal lengths, with their accompanying changes of image-scale. Those most frequently used are two off-axis parabolic mirrors of 16 inches (40.5 cm) and 12 inches (30.5 cm) diameter respectively, and of 40 feet (12.2 m) and 20 feet (6.1 m) focal lengths; from either of these the light is reflected up to a third flat in the tower and hence back to form an image of the sun on the first slit of the spectroheliograph.

Other elements are also available in this optical train, so that any one of the following five focal lengths may be used—50, 40, 20, 8.1, and 6.17 feet (15.2, 12.2, 6.1, 2.47, and 1.88 meters). The longer focal lengths are of service in studying the details in small chromospheric phenomena; the day-time seeing at Lake Angelus has proved to be so good that the forty-foot focal length is used in most of the prominence motion pictures. The need for sudden changes to shorter focal lengths comes when an eruptive prominence is under observation, and shorter focal lengths must be instantly available to keep the rapidly rising prominence within the motion picture frame. A good example of this need for such changes was afforded by the great eruptive prominence recorded in September, 1937. Although the entire life of this eruption was only about eighty minutes, three successive

changes of focal length were made to increase the area covered, and even then the extreme upper portions were lost when the prominence top reached an altitude of over one million kilometers above the solar surface.

All the mirrors of the optical train of this tower are of pyrex coated with aluminum, thus avoiding any absorption of the light in passing through a lens. Because of this feature of the design, the completed instrument is of exceptional speed; our current exposures on prominences range from ten to thirty seconds, where most other installations must reckon their exposure times in minutes instead of seconds. These short exposures thus give equally small time intervals between the mid-points of successive frames, and operate to make the projected film a continuous record for all practical purposes.

The solar image falls upon the first slit of the spectroheliograph; when working on prominences in profile at the limb of the sun, an occulting disk of the proper diameter with a carefully polished edge serves to cut out the solar disk up to the edge of the chromosphere; when working on features on the disk of the sun, the occulting disk is naturally removed. We now are employing in prominence work an occulting disk of neutral glass transmitting only 10 percent of the light instead of a metal disk; in this way, the projected picture shows not only the prominences in profile, but also any sunspots or flocculi near by on the solar surface, frequently of great value in locating possible centers of attraction for prominence streamers.

The two slits mounted on the heavy circular spectroheliograph head are made of carboloy, almost equal to the diamond in hardness; as a matter of fact these slits, one inch (25.4 mm) in length, had to be finished with diamond powder. Through a precision built link-work operated by a motor-driven cardioid cam, the slits are given precisely equal and opposite motions at the rate of two complete "scans" per second.

The spectroheliograph head, on which the slits, the special motion picture camera, and other parts are mounted, forms the upper end of a massive steel cage of about three and one-

half tons total weight that is located in the steel lined well beneath the tower; this cage can be rotated through 360° (the first spectroheliograph to be thus provided with a complete rotation) and is so precisely mounted on a large ball-bearing race beneath the circular top plate and a counter-weighted ball-bearing at the lower end that it can be turned with a finger. In this cage is located the 600-pound (272 kilogram) traveling carriage, operated by electric motor with push-button control, that carries the grating and collimating lens unit on vertical ways within the cage for focusing purposes. Our dispersive element at present is a 6-inch grating with ruled surface 4×4 inches (10×10 cm) and 15,240 lines per inch, on loan from Dr. Pettit's laboratory at Mount Wilson. It is expected that a grating will, eventually, be ruled for our special requirements.

We ordinarily work either at the K line of calcium or the $H\alpha$ line of hydrogen; beautifully clear and sharp hydrogen or calcium spectroheliograms have been taken with *both* slits 0.5 mm wide, and for some work with the second slit a full millimeter or more wide; these points should be kept in mind in connection with the discussion given later as to the impossibility of assuming any Doppler effect as essentially operative in our spectroheliograms of prominence streamers.

It is manifestly impossible to give any detailed description of all the devices that have been found essential for rapid and accurate work with this tower telescope and attached spectroheliograph and motion picture camera. Every effort has been made to design an instrument which should not only be the most rapid but also the most flexible and convenient in existence. Some idea of the measures taken to this end may be derived from the fact that over forty small motors, including several of the Selsyn type, are used in the tower, the spectroheliograph, and in the underground control room, all operated by conveniently assembled push-button controls. Every motion of instrumental parts for focal changes or collimation of optical elements, tilt or orientation of the grating, drive frequency control, etc., is thus made by the operator at his

observing station at the spectroheliograph head, by merely pressing the appropriate button.

One of the newest and most useful improvements over previous instruments is the scout camera, constructed by the writer from a design by E. Pettit. A third slit passes the light from the *H* line of calcium to a conveniently arranged scout camera. It is, of course, impossible to know what sort of a record is being impressed on the film during a run by visual means, in the calcium lines. But scout pictures are taken in calcium *H* every few minutes during an important run and at once developed in the convenient small dark room located adjacent to the spectrograph well and below the floor of the work room. These scout pictures make it possible to keep accurate track of the behavior of the instruments and of the particular prominence under observation.

Recent Results in Prominence Research

A portion of the first of the two reels shown in the presentation of these results before the American Philosophical Society was devoted to scenes showing the performance of the various mechanisms of the tower telescope and the spectroheliograph, as schematically described in the first part of this paper above. The balance of the motion pictures were of various interesting solar phenomena that have been recorded in the work of the past two summers. There are some respects in which these films are unique as scientific records, and attention may fittingly be called to some such characteristics before proceeding to a more detailed description.

1. The pictures are in a very real sense "modern" inasmuch as the first solar films taken with the new McMath-Hulbert tower telescope were made on July 2, 1936, one day after the completion of the tower. The scenes shown before the Society were mostly of the runs of the season of 1937, though some were from the preceding summer.

2. They are definitely unique in their field, because no other installation at present exists which has the instrumentation for similar motion picture records of solar phenomena.

3. They were the nearest approach to *continuous* records of solar surface changes that have ever been made, and in this factor lies perhaps the largest portion of their value as scientific documents for puposes of solar research. Photographs of the solar surface in white light, and spectroheliograms of solar prominences in the light of calcium or hydrogen have been taken for several decades, but most of these were effectively "stills." The nearest approximation to our work was that done by Dr. Pettit at the Yerkes Observatory with manually operated apparatus where exposure intervals of four or five minutes were maintained over several hours; the nearly continuous character of these new records taken at intervals of a few seconds shows, however, the changes *as they are taking place*, and not only makes possible a more detailed study of the mechanisms underlying the phenomena, but has also brought to light a mass of new details, hitherto unsuspected, and unrecorded in the still pictures of the past.

The films shown were a representative selection from over ten thousand feet of standard 35 mm film exposed in the new tower telescope on the solar prominences or on features of the solar disk itself during the seasons of 1936 and 1937. This new method of attack on the problems exhibited by the surface of a star is still too youthful to permit of explanations of each and every phenomenon involved; fresh puzzles, too, frequently show themselves in each run on a new prominence in the active stage, and doubtless the season of 1938 will bring to light as many new features as have the films taken during the past two years. For such reasons, and because we are still working on many of the puzzles presented, it is, in some cases, premature to advance any precise formulation of hypotheses, but in every way preferable merely to suggest certain lines of attack that are under consideration while additional corroborative data are being secured through observation and measurement. The essential portions of our researches on solar features, to date, are then assembled on the following pages, either in the form of summaries, or with a detailed description of outstanding individual phenomena

shown on the films that accompanied this paper in its presentation before the Society.

New Prominence Types

It has become necessary to add five new type sub-divisions to Pettit's classification of solar prominences.[1] Pettit's classification now reads, with these additions discovered at Lake Angelus:

Pettit's Classification of Solar Prominences

Class I. Active prominences.
Class II. Eruptive type prominences.
  IIa. Quasi-eruptive prominences (New).
Class III. Spot type prominences.
  IIIa. Broken filament.
  IIIb. Complete loops.
  IIIc. Incidental prominences associated with sun spots.
  IIId. Surges (New).
  IIIe. Coronal type prominences (New).
  IIIf. Ejections (New).
  IIIg. Cap prominences (New).
Class IV. Tornado type prominences.
Class V. Quiescent type prominences.

Of these new prominence types, those of the coronal type, tentatively classified as Class IIIe, are, perhaps, the most interesting, as well as the most puzzling. During the showing of the reels, the discovery scene of the coronal type was shown, which was taken on October 4, 1936, and the audience was requested to watch all succeeding scenes for these coronal prominences, many of which are quite faint and require the best of projection conditions to be easily visible. The coronal prominences appear to form at distances of 240,000 km or more above the chromosphere; it is, perhaps, more correct to say that the matter which forms them becomes luminous at this elevation through some mechanism the precise nature of which is still awaiting adequate explanation.

[1] *Astrophysical Journal*, **76**, 15 (1932); *ibid.*, **85**, 279 ff. (1937).

From the elevation where they first appear, the coronal prominences then pour down into some center of attraction in a long, faint, slender column which frequently is broken up into successive knots and which brightens as it approaches the chromosphere. Attention was also called to some interesting examples of prominences of Classes IIIa, and IIIb, somewhat similar to the coronal prominences but generally brighter, where the material seems to appear first in the shape of a bright luminous cloud which feeds arches or streamers down into the sunspot area. In Class IIIb, the motion appears to be prevailingly downward on both sides of the loop, there being no visible evidence of material moving up along one-half of a loop. Closely allied with such arched loops of lower height are the occasional "giant arches," which show the same curious phenomenon on a larger scale. At least one such giant arch has been recorded with its "feet" about 160,000 km apart on the chromosphere; although there appeared to be no noticeable accretion of matter at the summit of the arch, 150,000 km above the sun's surface, matter in the form of luminous knots was observed to be continuously descending along *both* branches of the arch. No adequate explanation of this peculiar phenomenon is, as yet, at hand, though it may be allied with the reason that something like 90 per cent of all the motions recorded for prominences not of the eruptive type are downward. Upward motions of matter seem to be confined to *eruptive* prominences, the *surges* which rise from and fall back into sunspot areas, and to the faint Roman-candle-like *ejections* which also rise from sunspot areas. Certainly the relatively sudden appearances of these high and approximately stationary luminous clouds of Class IIIa (and doubtless also the arch phenomena of Class IIIb) strongly suggests the existence of a chromospheric solar atmosphere, though eclipse evidence as to the existence of such an atmosphere is entirely negative.

The surges of Class IIId are a type that has never before been recognized, perhaps because they are prevailingly very short-lived. Smaller and more frequent examples resemble

sharp-pointed spearheads of flame that stab upward to heights of 5,000 to 15,000 km and as rapidly subside again, with a total life-period of only a few minutes. As seen in profile-runs on prominences over sunspot areas, the limb of the sun will occasionally exhibit an almost continuous activity of this type. Occasionally, the surges will be much larger, as in the discovery picture of the great Class IIId surge of August 8, 1936, which was concurrently photographed in both calcium K and hydrogen α, and shown on the screen in both these elements. It is interesting to note that this great surge which rose to an elevation of 80,000 km coincided almost exactly with a radio fade-out reported by Dellinger of the Bureau of Standards.

Several scenes showing a spot group through the 10 per cent transmission occulting disk mentioned earlier, were also projected. In these pictures, the spot ejects a long streamer of material which remains stationary at its maximum height for a short period of time and then the same material, apparently, returns into the spot without breaking continuity along the same trajectory, as in several giant surges we have photographed. This is evidence that giant surges may come directly from an individual sunspot and was first observed during the summer of 1937.

Dark Hydrogen Flocculi

Previous investigation has shown little, if any, internal motion in the dark hydrogen flocculi seen in projection against the solar disk. We have been fortunate enough to observe for a total elapsed time of $5\frac{1}{2}$ days an enormous, long, dark hydrogen flocculus which extended well across the disk of the sun and had a total length of about 1,000,000 km; this flocculus was in reality a tremendous prominence projected on the disk. Its life history, its internal motions, and its final disintegration when pulled apart by centers of attraction at the end of its life of over five days were clearly shown on the screen. So far as is known, this is the first case where one of these dark hydrogen prominences has been followed from its initial

appearance till its dissolution. Such flocculi are simply prominences; they exhibit internal motions and show the characteristic streamer formation and their projection into centers of attraction.

Numerous other instances have been recorded, and several examples of prominences projected against the solar disk taken with the spectroheliograph at a dispersion of 1.8 A per mm obtained with the collimator of thirty feet (9.1 meters) focal length were shown on the screen. Such runs show the chromosphere to be in a continuous state of turmoil, and streamers can be seen leaving the prominences and entering centers of attraction, though no visible marking is evident to identify such centers.

The large sunspot group of the summer of 1937 was also shown; this was among the five largest spot groups ever photographed. This showed quite clearly the large number of bright chromospheric eruptions of very short duration the nature of which is not yet established. These scenes also demonstrated a well-marked tendency toward simultaneous eruptive activity throughout the entire area of the disk under observation.

Eruptive Prominences of Class II

Several interesting cases of prominences of the violently eruptive type were illustrated. Noteworthy among these was the great eruptive prominence of September 17, 1937, which finally left our motion picture frame (after three successive changes to shorter focal lengths!) at a height of about 1,000,000 km. For a few months, this prominence held the world's record as to observed height but has since been surpassed in a prominence recorded by Hickox at Mount Wilson on March 20, 1938, which rose to a height of 1,550,000 km. In the September prominence photographed at Lake Angelus, the velocity of the "head" blown off from the main part of the prominence reached a value of 728 km/sec. This is, so far as is known, the greatest velocity ever measured in a solar prominence. At the distance from the sun reached by

this blown-off prominence head, the recorded velocity is considerably greater than the "velocity of escape" which exceeds the attractive power of the sun's gravitation. Thus this is believed to be the first recorded instance where we have observed matter actually shot out into space beyond the sun's attraction, though such possibilities have long been recognized in theory.

Confirmation of Pettit's Laws of Prominence Motion

Through the study of the motions of many prominence streamers, abundant evidence has been secured in confirmation of Pettit's curious and still somewhat mysterious laws of prominence motion.[1]

These laws may be formulated:

Pettit's Laws of Prominence Motion:

Law I. The motion of a portion of a prominence or of the prominence as a whole measured along its trajectory is uniform, increasing suddenly at intervals as if by an impulse.

Law II. When a change of velocity takes place, the new velocity is a small whole multiple of the old. Occasionally, the *last* velocity is a whole multiple of the second previous.

These laws were first found to apply to eruptive prominences, but later it was shown, largely by the work at Lake Angelus, that they apply to all prominence motions. To illustrate, the velocities observed in the great eruptive prominence of September 17, 1937, before mentioned were 28, 58, 186, 540, and 728 km/sec. Here it is to be noted that the second velocity is twice the first, the third three times the second, the fourth three times the third and, while the fifth and last is only one-and-a-half times the fourth, it is nearly four times the third.

The motions of knots and streamers do not ordinarily exhibit so many changes of velocity as the eruptive prominence cited above, frequently none. The active prominence

[1] Pettit, *Astrophysical Journal*, **76**, 21 (1932). *Pub. Yerkes Observatory*, **3**, pt. IV (1925).

of May 11, 1937, showed velocities of one knot of 14, 42, and 84 km/sec. The second velocity is three times the first and the third is twice the second. A loop in the sunspot prominence of August 1, 1937, gave velocities of 32.4, 64.8 and 128.0 km/sec; each succeeding velocity is double the previous one. One of the coronal prominences of October 4, 1936, gave velocities of 52.6 and 158.0 km/sec, and another 28.0 and 148.8 km/sec, the second velocity being respectively three and five times the preceding. The coronal prominences of June 6, 1937, gave for the first case velocities of 115 and 232 km/sec, a ratio of one to two, while two succeeding prominences gave 168 km/sec without change of velocity.

We may amplify and illustrate these strange laws of prominence motion somewhat as follows. We should expect that the motion of a knot along the trajectory of a streamer would follow the laws of the motion of a particle in a conic section under the law of gravitation; that is, that an acceleration would be observed as the knot descends toward the center of attraction in the solar surface. But under Law I, we find that the motion is uniform, nor can any gradual acceleration of velocity be detected. Again, when this uniform velocity is changed to a new uniform velocity, the change takes place suddenly and without apparent intervening velocity gradient; the new velocity will nearly always be some simple multiple of the former velocity. For example: if a knot has been descending at a uniform rate of 23 km/sec it will suddenly (frequently in less than a minute of time) change abruptly to a new uniform velocity of 46 km/sec; should a third acceleration take place it will generally be to 92 km/sec, or even to 138 km/sec under Law II.

To say that these curious laws of prominence motion are mysterious is very definitely an understatement. They contradict every known law of the motion of particles under the action of a gravitational attracting center; it is as though acceleration in prominences functioned only in definite "acceleration-quanta." The very large amount of measurement that has now been made upon the motions of knots and other

formations in prominence streamers of all types corroborate Pettit's Laws of Prominence Motion beyond the shadow of a doubt. But an acceptable theory of these apparently almost instantaneous integral changes in velocity will doubtless be far more difficult than has been the collection of the observational data bearing thereon.

Predominance of Matter in Descent

It is a very curious fact, still awaiting an adequate theory, that even if we include matter shot upward in the prominences of violently eruptive types, noted earlier, at least 90 per cent of our records of prominence material in motion show matter *in descent only*. On a number of great "banyan-tree" prominences with multiple stalks or trunks connecting the enlarged upper portions to the chromosphere, bright nodules of matter are shown spiralling downward along the "trunks." We have mentioned earlier the frequent growth of luminescence in existing matter or the actual formation of faint luminous clouds high above the sun from which the coronal and other streamers of Class IIIa descend, phenomena which seem to necessitate the postulation of some form of chromospheric atmosphere mixed with the corona. This may be local, in the form of pockets of gas, but of this we must search for more evidence.

Much the same type of phenomena are exhibited in the lower bright arches and loops of prominences of Classes IIIb and IIIc. In these beautiful, fountain "set-pieces" showing a multiple spray effect, the motions of *descent* are always clear, brilliant, and rapid, while any corresponding *ascent* of matter where the *rising* side of the completed trajectory should be is either so faint that no motion can be detected or, as is more frequently the case, it is entirely absent.

As will be noted later under the appropriate heading, we are utterly unable to explain this apparent absence of matter in ascent through any application of the Doppler effect.

The phenomena still remains a puzzle. It is quite apparent that what comes down in great probability has formerly

gone up in some state or other, but why should such upward motions be invisible? Can it be that the hot gases are in some different temperature or ionization state on their upward path, changing back to another and photographically recordable state soon after passing the crest of their trajectory? Are the luminous and nearly stationary bright clouds that seem to form, or at least to become first visible, at heights of one or two hundred thousand kilometers above the solar surface and then to descend in long narrow streamers, another manifestation of the same general process? We have no very satisfactory answers to these questions as yet, though we are trying out a number of possible hypotheses and collecting additional data that may, eventually, furnish an answer to this pressing and difficult problem.

The Doppler Effect and the "Reality" of Prominence Phenomena

Astronomers who see these motion pictures for the first time frequently try to explain the marked predominance of matter in descent by invoking the Doppler effect. At first sight this would seem to be a possible cause, but a more careful examination of all the instrumental data involved renders the theory untenable, in our opinion. Were such an explanation valid, there would be a serious element of unreality in our motion pictures of prominence phenomena, for the assumption would be that our grating spectrum was so set as to record only the descending half, in general, of a loop trajectory. Under the assumption that the ascending half of the trajectory differed sufficiently in radial velocity from that of the descending half to move the picture of the ascending portion completely off the second spectroheliograph slit, we would, of course, obtain a picture of only the descending portion; the ascending portion, though materially present, would be invisible in the spectroheliogram and our final picture be, in effect, only "half a picture."

It is patent, of course, that there are limits to the radial velocities that can be included, depending upon the slit width

used and the constants of the instrument. We are willing to admit, without argument, that a feature possessing a radial velocity 100 or more km/sec different from the average velocity in the line of sight to which our grating was set would be missing from our spectroheliogram of the scene. Indeed, it would also be possible for such a feature possessing intrinsically a wave-length slightly different to come into the picture as a new and extraneous feature. We have, in fact, noted only two cases in which the Doppler effect seemed to be operative in bringing in such an extraneous feature. Attention was called to these cases in showing the films before the Society, as they would ordinarily be missed by one seeing these films for the first time; both of these cases took the form of a sudden almost explosive brightening of small "patches" near the chromosphere.

We have devoted not a little time in experimenting with different slit widths in the effort to reach a definite conclusion as to the possible contributions of the Doppler effect; it is our judgment that this effect is inoperative in the vast majority of our scenes, and that our films show a picture of prominence activity that is "true." The slit width used for much of our earlier work in Ca+ was 0.40 mm, equivalent about to 1.4 A with our dispersion. This means that a radial velocity of about $\pm$ 100 km/sec would be required to produce a serious displacement of the line from the second slit. In some later work in hydrogen, we have used slits 0.5 mm in width; in this case there would have to be a difference of radial velocity of $\pm$ 85 km/sec to cause a harmful shift. The last scene of the two reels shown before the Society was a wide-slit picture of this sort—a beautifully clear and sharp scene of streamers recorded in the light of Hα descending slowly to the chromosphere, taken with both slits 0.5 mm in width. In the middle of the travel of a knot at the top of a loop, the velocity along the trajectory seldom exceeds 40 or 50 km/sec, or a speed very considerably less than that necessary to cause an obliterating shift of the line. While velocities as high as 150 km/sec (about the maximum value observed) are occasionally ob-

served along a trajectory, these high velocities occur after several increases at the extreme end of the loop as it enters the chromosphere, where the component is nearly radial to the sun and thus perpendicular to the line of sight.

A further and, apparently, conclusive argument against the adequacy of the Doppler effect to produce an "unreal" picture may be derived from an elementary geometrical consideration of the orientations of the loops. In a large and active prominence of Class IIIa, loops are observed in all possible orientations to the line of sight; some with planes lying nearly in the line of sight, and others the trajectory planes of which are roughly perpendicular to the line of sight. Were the Doppler effect operative, we should expect radical differences in behavior and appearance. But all such loops, whatever their orientation, show the same record of matter in descent only, with the opposite branches (where ascending matter might be expected) extremely faint or entirely invisible.

Furthermore, since the normal setting is kept intact throughout a scene, we would expect that in loop formations only that part of the rising trajectory up to a radial velocity of about 114 km/sec would appear in the picture, and that it would be the latter half of the picture, from the crest to where the knot re-enters the chromosphere, that would be lost. But just the reverse condition obtains; it is the descending half only that we see, while there is no ascending half visible.

Again, we have tested the effects of maladjustment on a Class IIIb prominence by taking exposures with the line in the normal position, followed by a short run first with the line displaced 1 A to one side, and then the same amount to the other. When these three sets of frames were compared, no essential difference was noticeable save in the sharpness of image, which is, of course, best for the normal line position.

With the Doppler effect rather decisively ruled out as a cause of the observed predominance of matter in descent, our puzzle, apparently, remains unsolved. An electrical field about a spot area that operates to condense a very tenuous

atmosphere and pull it back into the chromosphere has been suggested, but meets with some theoretical difficulties, though these may not be insurmountable eventually.

Conclusion

In this necessarily brief presentation of the results of our researches on solar phenomena, we have outlined some of the results, and included some of the puzzles, that are growing out of the application of this new technique to this particular field of astronomical photography. The close continuity of our solar records, as projected on the screen, bids fair to solve some of our previous problems, just as it has disclosed numerous phenomena of the sun the existence of which was not previously suspected. Along with such contributions, as in any worthwhile branch of research, it has, doubtless, brought a number of new problems greater than those for which it affords some explanation. The work is being continued as fast as money and time will permit, and we realize only too fully that only the beginning has been made. In particular, from the known connection between the number of sunspots and the activity of flocculi and other disk features, we realize that the results secured during the sunspot maximum through which we are now passing must be balanced by motion pictures of solar activity taken during a sunspot minimum. Certainly only a beginning of our present program of solar research will have been made until we work completely through, at least, one sunspot cycle.

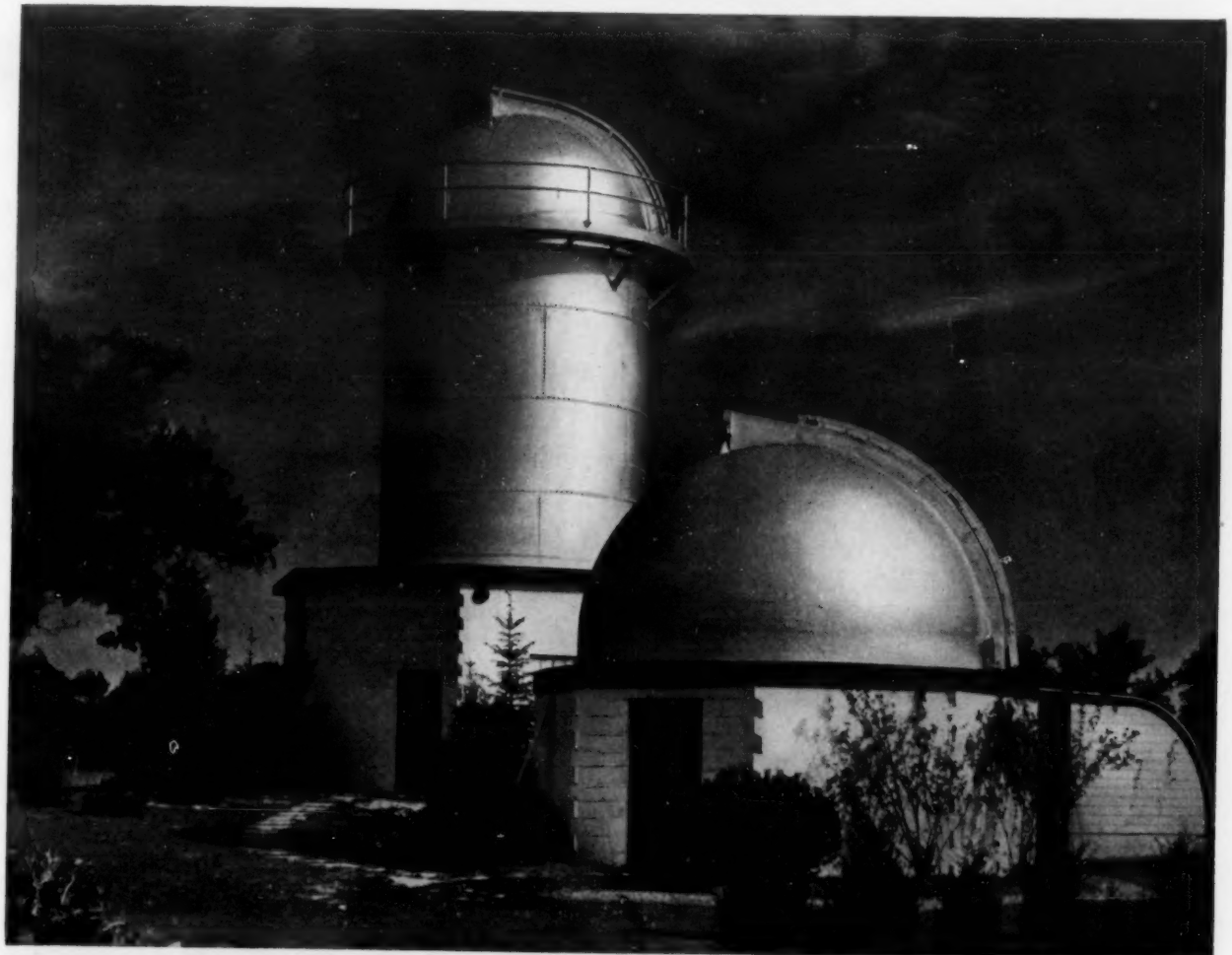


PLATE I. McMath-Hulbert Observatory of the University of Michigan—Lake Angelus, Pontiac, Michigan, from a photograph by Sidney D. Waldon. (Dome in lower right foreground houses the spectroheliokinematograph. Dome in center back covers the cœlostat and tower telescope.)

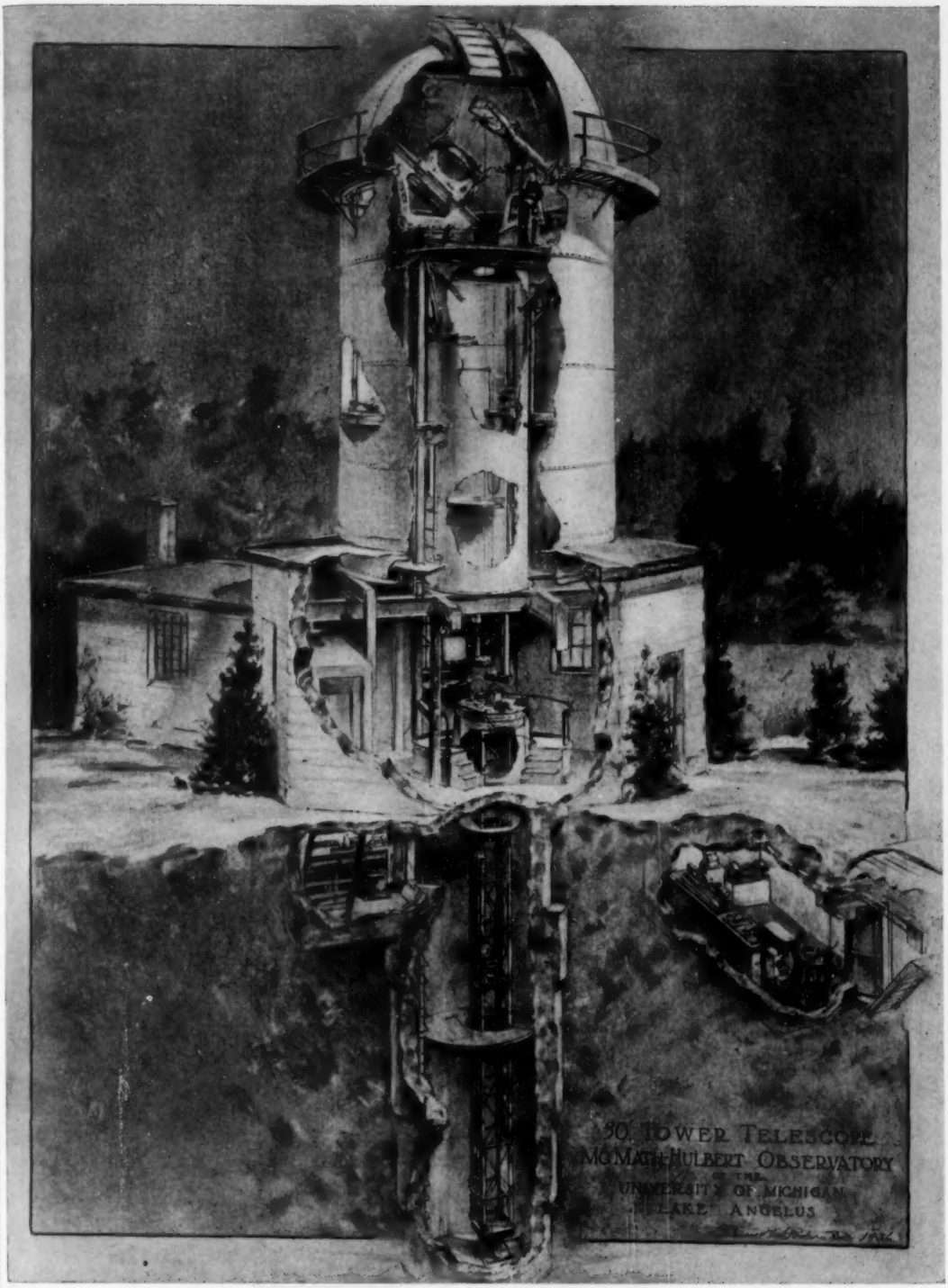


PLATE II. The 50′ Tower Telescope of the McMath-Hulbert Observatory from a drawing by Russell W. Porter.

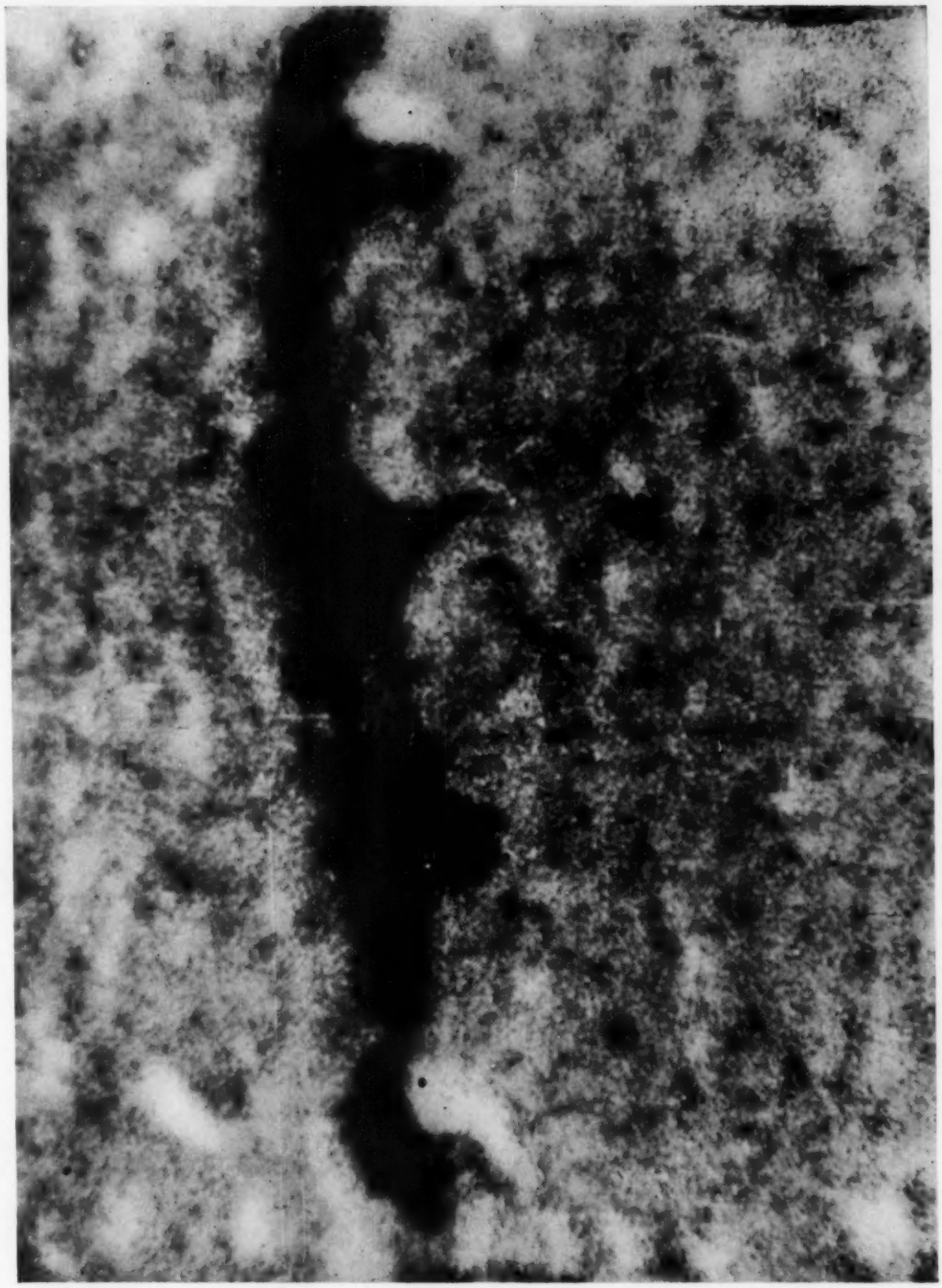


PLATE III. Part of great prominence as seen projected upon the sun (dark flocculus) photographed on motion picture film in the light of $H\alpha$ July 5, 1937. The matter in the tooth-like projection on the lower side is moving into the sun.

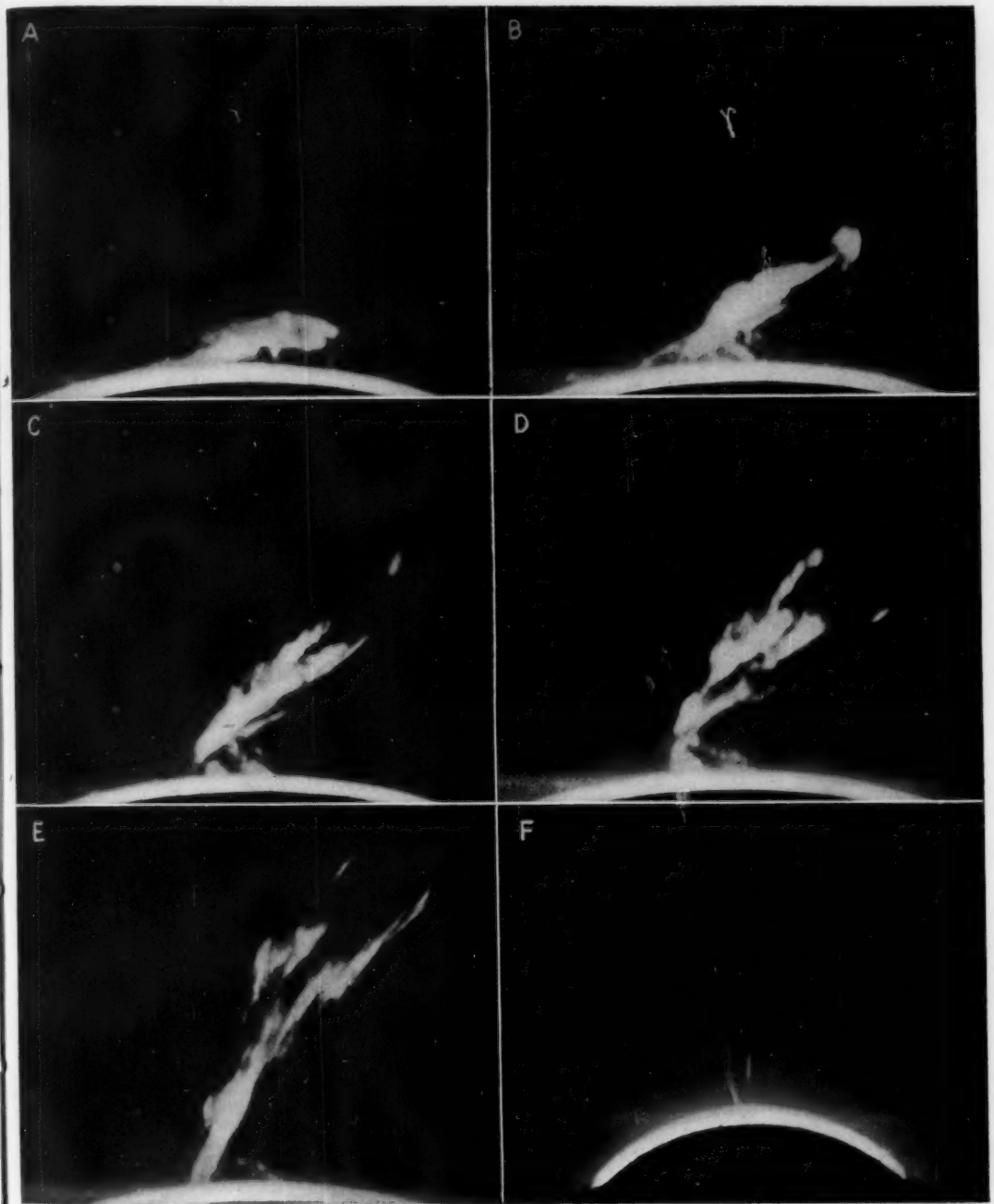


PLATE IV. Great eruptive prominence of September 17, 1937, photographed at the McMath-Hulbert Observatory.

A—14^h50^m69; B—14^h55^m84; C—15^h06^m13;
D—15^h09^m11; E—15^h14^m31; F—16^h06^m7 G.C.T.

Exposures A to E with 20-foot focus mirror, F with lens of 74 inches focal length.
In F the prominence goes out of the picture 1,000,000 km above the sun.

PHOTOGRAPHIC METEOR STUDIES, I

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ABSTRACT

Radiants, heights, velocities and decelerations have been measured for six meteors photographed simultaneously with two cameras, one equipped with a rotating shutter. The velocity measured for a Perseid shower meteor agrees well with the known velocity of the associated comet. A Geminid is found to have been moving in an orbit with period 1.8 years. Of four sporadic meteors studied, three are found to have been moving in short-period orbits with very small inclinations to the plane of the ecliptic. The fourth sporadic meteor was moving with retrograde motion in a nearly parabolic or possibly hyperbolic orbit.

The measures of deceleration coupled with measures of velocity and luminosity provide a rough determination of the fraction of the relative kinetic energy of the air molecules available for heating the nucleus of a meteor. The theories by Öpik and Hoppe are the fundamental basis of these calculations and for determinations of atmospheric densities at heights from fifty-eight to ninety-nine kilometers above the earth's surface. A density-distribution curve in the atmosphere is derived from other physical observations; the densities determined from the meteor data are compared with the values from this curve. A seasonal effect with a total amplitude of 0.5 in log density is suggested by the observations.

Formulæ are derived for the determination of meteor velocities from less complete observational material.

THE methods of photographing meteors for the determination of trajectories, velocities and decelerations are well-known, simple and effective. The results obtained by these methods during fifty years of intensive astronomical photography are, nevertheless, astonishingly meager. Thorough application of meteor photography should eventually yield a definitive knowledge of the sources of bright meteors, a better understanding of the physical transformations occurring when a meteor enters the earth's atmosphere, and more direct information concerning the physics of the upper atmosphere.

In the present paper are presented the results obtained from a study of the trails of the first six meteors photographed in a two-camera meteor program recently instituted at the

northern stations of the Harvard Observatory. The paper is divided into sections as follows: (1) Instruments; (2) Method of determining the apparent radiants; (3) Methods of calculating the heights, velocities, and decelerations; (4) Observational results; (5) Discussion of astronomical results; (6) Magnitude determinations; (7) Physical theory; (8) Numerical values of the constants; (9) Density of the atmosphere and other physical results.

A timed rotating shutter in front of one of the two cameras necessary for satisfactory meteor photography was first used extensively by the late Professor Elkin of Yale in a meteor program carried on during the years 1893 to 1909. The results of this program have been compiled and augmented by Olivier.[1] The base line used was, unfortunately, too short for the accurate determination of meteor heights and velocities. Lindemann and Dobson [2] were apparently successful in obtaining simultaneous photographs of meteors from two stations and in determining velocities by means of rotating shutters, but their published results are scanty. Fedynsky and Stanjukovitch [3] photographed two meteors with complete equipment, and determined the spacial orbit of one. Whitney,[4] using synchronized shutters on the two cameras, photographed one meteor.

At Harvard, special cameras equipped with rotating shutters were used by the late Dr. W. J. Fisher for the study of shower meteors. At the author's suggestion, the AI patrol camera at the Oak Ridge station was also equipped with a rotating shutter in the summer of 1935. All of the earlier meteor photographs taken with these instruments have been discussed by Millman and Miss Hoffleit.[5] The AI meteor program has been recently augmented by the installation of a similar patrol camera (FA) in Cambridge. Since February 15, 1936, the observing programs of the two cameras have been

[1] *A. J.*, **46**, 41, 1937.
[2] *M. N.*, **83**, 163, 1923.
[3] *Russ. A. J.*, **12**, 440, 1935.
[4] *Pop. Ast.*, **45**, 162, 1937.
[5] *H. A.*, **105**, 601, 1937.

synchronized for the simultaneous photography of meteors, with the result that seventeen pairs of meteor trails have been photographed in twenty-seven months. The measurements and reductions of six pairs have been completed, of which two were shower meteors (a Geminid and a Perseid), and the others were sporadic.

1. Instruments

The AI patrol camera has a Ross Xpress lens of focal length 5.95 inches, and aperture 1.5 inches. The field of sharp definition is about sixty degrees. The shutter of two blades is rotated by a synchronous motor made to order by the Warren Telechron Co. of Ashland, Mass., to give twenty interruptions per second (mean solar time), the lens being covered approximately one ninth of the time. A test * in February 1937 by Mr. Fletcher Watson and Mr. Frederick Ireland indicated that the rate was maintained constant to one part in one thousand, in spite of winter temperatures.

The FA patrol camera is equipped with a similar Ross Xpress lens of focal length 5.99 inches. The field of fair definition subtends an angle of more than sixty degrees, with the central fifteen degrees somewhat out of focus.

The routine observing programs are synchronized so that the two cameras are directed toward a point in space about eighty kilometers above the earth's surface. No meteor trail to date has been missed because of imperfect synchronization or because its height was such as to project the trail on the usable field of one camera and off that of the other. The base line of thirty-eight kilometers is great enough that the sky regions of simultaneous exposure with the two cameras are separated sufficiently to avoid much duplication in sky patrol programs. During the dark of the moon an AI plate is exposed for two hours while two FA plates on the corresponding region are exposed one hour each. Because of the presence of city lights at Cambridge, the FA plates receive a background fogging comparable to that of the AI plates exposed twice as

* By means of an adjustable stroboscope, made by the General Radio Co.

long at Oak Ridge. The FA is not operated during periods of bright moonlight.

In twenty-seven months of observing with the two cameras, two thousand FA plates were exposed. The photographing of seventeen meteor pairs in two thousand hours of exposure time is only slightly less than was hoped for from the previous experience of one meteor per one-hundred hours of exposure with a single camera. Observing at the two stations was completely independent except for the pre-arranged programs. As a result, simultaneous differences in sky conditions on poor observing nights permitted the exposure of a number of unmatched plates.

The geographical latitude and longitude of the FA camera were measured by offset from the 15″ refractor in Cambridge. The values adopted are: $\lambda = +71^\circ\ 7'\ 47''.28$; $\varphi = +42^\circ\ 22'\ 48''.4$; and altitude above sea level = 18 m. The position of the AI camera is, at present, less certain. From readings of United States Coast and Geodetic survey maps the following values were adopted: $\lambda = +71^\circ\ 33'\ 33''.0$; $\varphi = +42^\circ\ 30'\ 13''.0$; and altitude above sea level = 190 m. The adopted altitude is slightly too great. The equatorial rectangular coordinates of the FA camera in respect to the AI camera at zero hours sidereal time at Oak Ridge become: $X = +9.000$ km; $Y = +35.362$ km; and $Z = -10.243$ km. The resulting straight line distance between the two cameras is 37.9 km. The effect of the earth's oblateness was allowed for in these calculations. Triangulation measures of the Oak Ridge station are now being made by the Coast and Geodetic Survey. When the results are avilable the relative positions of the stations should be known to five instead of to three significant figures.

2. Method of Determining the Apparent Radiants

Only an outline of the methods of reduction used in the present investigation will be presented here. The general principles are well known and the detailed formulae can easily be derived to fit the needs of an investigator. Direction co-

sines were used throughout in the numerical calculations, instead of trigonometric functions and angles.

On each plate, nine to twelve faint A.G. stars were selected, near to, and as symmetrically as possible with respect to, the meteor trail. The plate was placed in the Coast and Geodetic Survey measuring engine with the meteor trail aligned parallel to one of the two perpendicular screws. Let x represent the running coordinate of the trail and y the coordinate perpendicular to the trail. The x-coordinates of all irregularities in the trail were measured at least three times, with three to five settings to each measurement in the case of the AI plates, and at least twice for the FA plates. The y-coordinates were measured at ten to twenty values of the x-coordinates. The coordinates of the stars were measured enough times to reduce the setting error to below two microns. Occasional gross errors in the x-measures of the shutter breaks arose from the influence of the closely spaced star images and could not be reduced by repeated measurements. Measures were made at the central points of the breaks, the ends being very difficult to set on accurately.

Because of the great extent of some of the trails (more than 18 cm or 30°) it seemed important both to allow for and to determine the amount of the lens distortion (deviation of the lens projection from point projection). Millman and Miss Hoffleit [5] reduced the AI plates on the assumption that the lens was distortionless. To test for distortion, three stars were chosen symmetrically about the apparent centers of the plates with the longest trails. These stars were measured along with the meteor trail stars and each plate center was determined by the method described by Miss Olmsted.[6] With this plate center standard coordinates [7] were determined for all the stars and were related linearly to the measured coordinates by means of the three centrally-symmetrical stars. From the linear relations so derived, theoretical coordinates for each star measured on the plate could be calculated. The differ-

[6] *H. A.*, **87**, 221, 1931.
[7] *See* Turner, *M. N.*, **60**, 201, 1900.

ences between these theoretical coordinates and the observed coordinates are the result of lens distortion, screw errors, errors of measurement, errors in star positions, and the effects of differential aberration and refraction. Of these, distortion and refraction are by far the most important.

When the effect of refraction had been eliminated in one reduction there still remained a definite distortion effect amounting to over a hundred microns in thirty degrees. An error of this amount would be very serious in accurate position work but almost negligible in calculating the velocity of a meteor from shutter breaks as done by Millman and Miss Hoffleit.

In the present investigation an empirical correction curve was drawn relating the difference between the theoretical and measured x- and y-values to x. The measured values of x and y were corrected by this curve, to reduce them to the standard plane determined by the plate center. This procedure allows accurately for distortion and systematic screw errors. Proper motion corrections were not applied to the star positions. The use of mean positions of the stars eliminates light aberration effects (except the constant term) and the effects of refraction, except for the exceedingly small amount that occurs in the atmosphere beyond the height of the meteor. Precession was treated in the ordinary manner. Light time, however, was neglected in all the calculations.

Because the corrected coordinates of points on the meteor trail are referred to the theoretical plane determined by the adopted plate center, the corrected x's and y's should lie on a straight line if the meteoroid moved in a straight line (apparent great-circle motion) in the atmosphere. A least-squares solution was made in each case to determine the line of motion on the theoretical plane. The deviations were usually negligible, but in one case amounted to a few seconds of arc, of the same order of magnitude as the free fall of the meteor due to the earth's gravitational attraction during the interval of observation.

After the least-squares solution for the line of motion on the theoretical plane, this line was transformed, by the linear

equations already derived, to standard coordinates and hence to the direction cosines of the pole of the meteor's great-circle motion as seen from the station. The direction cosines of the apparent radiant were then derived for a given meteor by solving for the pole of the great circle passing through the poles of the meteor's motion as seen from the two stations. Simple check formulæ were used throughout the calculations. The equatorial coordinates of the apparent radiant so derived are independent of the exact time of the meteor's apparition because the cameras were set to follow the diurnal stellar motions. In no case were the star images on the plates appreciably trailed. The directions of the radiants were in all cases determined to an accuracy of the order of two minutes of arc.

3. Methods of Calculating the Heights, Velocities, and Decelerations

If the time of a meteor's apparition is known, the determination of the height at any point on the trail is a rigid geometrical process involving the apparent trails as seen from the two stations. Only one meteor of the six here discussed was observed visually. It was necessary to determine the apparition times of the other five meteors by identifying natural irregularities in the trails as seen from the two stations. With this material the true time could be determined accurately for two meteors, satisfactorily for another, and uncertainly for the other two. It is the author's opinion that in most cases where the trails are reasonably intense on the photographic plates, enough natural irregularities occur for satisfactory time determinations. The work involved in the measurements, identifications, and reductions is, however, tedious and time consuming, and justifies considerable effort toward its elimination by one of the several fairly simple methods that can be devised.

The meteor heights were determined by a rectangular-coordinate method which depends upon the intersection of the two planes passing through the stations.[8] The method

[8] For Schaeberle's method *see* Olivier, *Meteors*, 1925.

does not depend upon the identification of points on the two observed trails, except for the determination of the time of apparition.

The corrected coordinates of the shutter breaks on the theoretical camera plane were transformed to distance, s, along the actual trail in space by the expeditious method presented by Millman and Miss Hoffleit.[5] These distances in km are given in Table I. The weights are based on the appearances of the breaks at the time of measurement. Exclusions were made only when the trail was too faint for certain identification of the break or when the presence of bright stars made a measurement obviously unreliable. The effects of a luminous train on any of the measures could not be detected, though such trains certainly did exist in some instances.

Three types of equations were fitted to the measured distance of each meteor in Table I by means of least squares, the measures being weighted as indicated. The type forms of these equations are:

$$s = A + Bt + Ct^2; \tag{1}$$

$$s = A + Bt + Ct^3; \tag{2}$$

$$s = A + Bt + Ce^{dt}, \tag{3}$$

where t is the time in mean seconds, A, B, and C are unknown constants, and d is a constant arbitrarily chosen for each meteor. The deviations from uniform motion were found to be so small that a solution for a fourth unknown reduced the weights of the derived constants to such an extent that no significant solutions could be made. Thus it was not possible to solve for the constants of a power series in the time complete even to the cubic term. One should expect that, of the three equations, No. 3 should most accurately represent the motion of a meteoroid that is moving nearly uniformly in an atmosphere in equilibrium with gravity. The approximately exponential increase of atmospheric density with decreasing height should produce an exponentially increasing deceleration. The constant d was determined for each meteor as

follows: the atmospheric density in the idealized atmosphere determined by Perkins [9] was calculated at the first and last breaks of the trail; the ratio of the last to the first density was multiplied by two and set equal to e^{dt}; d was determined from this equality in which t represents the time between the first and last breaks. This procedure is consistent with an assumption that the air resistance is proportional to the air density. The additional factor of two was inserted to allow for increased resistance arising from loss of mass by the meteoroid; one would not expect the factor to be actually as large as two, but this value was chosen as an extreme case.

For each meteor (identified by its file number in the Harvard collection of meteor photographs; see Fisher and Miss Olmsted [10] and Miss Hoffleit [11]) the measured distances (in km) along the trail are given in the third column of Table I. The first and second columns contain, respectively, the time in mean seconds from the first break and the square root of the weight assigned to the measure. The last three columns contain the residuals obtained from least-squares solutions for the constants of Equations (1), (2) or (3). The probable errors of observations of weight unity are given for each meteor and each equation at the bottoms of the partial columns of residuals. In Table II the constants, calculated according to the three equations as listed in the first column, are given in the second, third, and fourth columns, with probable errors. For the exponential equation, d is given in the fifth column. Referring to the mid-moments (t_0) of the observed time intervals, the calculated velocities (V_0) and the accelerations (V_0') are given in the seventh and eighth columns respectively. The ninth column contains the ratios of V_0' to V_0.

From the probable errors given in Table I, it is readily seen that the three equations fit the observations almost equally well. For Meteor 660 the probable error is smaller for the quadratic equation, but, in general, there seems to be no

[9] *Proc. Roy. Soc.* A, **158**, 650, 1937.
[10] *H. A.*, **87**, 233, 1931.
[11] *H. A.*, **82**, 179, 1937.

TABLE I

TRAIL COORDINATES AND RESIDUALS

Meteor 642

t	$\sqrt{wt}$	s	$O-C$ Eq. 1	$O-C$ Eq. 2	$O-C$ Eq. 3
s		km	km	km	km
0.00	0.3	0.0000	−0.0594	−0.0795	−0.0832
0.05	0.8	1.6339	+0.0098	+0.0037	+0.0014
0.10	0.8	3.1884	+0.0065	+0.0087	+0.0082
0.15	0.8	4.7376	+0.0046	+0.0105	+0.0119
0.20	1.0	6.2696	−0.0077	−0.0015	+0.0010
0.30	1.0	9.3358	−0.0097	−0.0090	−0.0071
0.35	0.8	10.8697	+0.0004	−0.0025	−0.0024
0.40	0.8	12.3916	+0.0052	−0.0003	+0.0024
0.45	0.8	13.9083	+0.0116	+0.0056	+0.0018
0.50	0.8	15.4094	+0.0091	+0.0059	+0.0026
0.55	1.0	16.8928	−0.0042	−0.0004	+0.0012
0.60	0.3	18.3470	−0.0399	−0.0238	−0.0094
			±0.0067	±0.0067	±0.0065

Meteor 660 (Sid. Time = $9^h\ 50^m$)

t	$\sqrt{wt}$	s	$O-C$	$O-C$	$O-C$
s		km	km	km	km
0.00	0.4	0.000	+0.023	−0.061	−0.080
0.25	0.7	3.429	−0.012	−0.026	−0.036
0.50	0.7	6.831	−0.031	−0.009	−0.010
0.75	0.7	10.277	+0.036	+0.068	+0.077
1.00	0.7	13.582	+0.004	+0.028	+0.042
1.25	1.0	16.880	+0.008	+0.016	+0.026
1.50	1.0	20.132	+0.009	−0.001	+0.001
1.75	1.0	23.310	−0.022	−0.042	−0.051
2.00	1.0	26.484	−0.015	−0.028	−0.040
2.25	1.0	29.641	+0.018	+0.035	+0.042
			±0.013	±0.022	±0.028

TABLE I (*Continued*)

Meteor 651

t	$\sqrt{wt}$	s	$O-C$ Eq. 1	$O-C$ Eq. 3
s		km	km	km
0.00	0.4	0.000	−0.030	−0.065
0.05	0.7	1.870	−0.018	−0.030
0.10	1.0	3.751	+0.012	+0.018
0.15	0.7	5.601	+0.018	+0.036
0.20	0.4	7.425	+0.006	+0.028
0.25	0.4	9.341	+0.092	*
0.35	1.0	12.864	−0.022	−0.010
0.40	1.0	14.664	−0.030	−0.027
0.45	1.0	16.523	+0.029	+0.023
0.50	0.7	18.293	+0.005	−0.004
			±0.017	±0.016

Meteor 663

t	$\sqrt{wt}$	s	$O-C$	$O-C$
s		km	km	km
0.00	1.0	0.000	−0.019	−0.027
0.05	0.3	3.949	−0.063	−0.068
0.10	1.0	8.004	−0.001	−0.002
0.15	1.0	11.997	+0.001	+0.003
0.20	0.3	16.069	+0.083	+0.086
0.25	1.0	20.003	+0.028	+0.032
0.30	1.0	23.963	0.000	+0.005
0.35	0.6	27.991	+0.041	+0.047
0.40	0.6	31.957	+0.021	+0.026
0.45	0.3	35.892	−0.028	−0.024
0.50	1.0	39.922	+0.018	+0.022
0.55	1.0	43.896	+0.010	+0.012
0.60	1.0	47.857	−0.010	−0.010
0.65	1.0	51.783	−0.064	−0.065
0.70	1.0	55.767	−0.059	−0.061
0.75	0.3	59.896	+0.092	+0.089
0.85	1.0	67.853	+0.097	+0.094
0.90	1.0	71.703	−0.027	−0.029
0.95	0.6	75.685	−0.019	−0.018
			±0.025	±0.026

TABLE I (*Continued*)

Meteor 670

t	$\sqrt{wt}$	s	$O-C$ Eq. 1	$O-C$ Eq. 3
s		km	km	km
0.00	0.4	0.000	−0.034	−0.043
0.05	0.4	1.300	+0.046	+0.044
0.10	0.7	2.459	−0.010	−0.008
0.20	0.4	4.940	+0.059	+0.063
0.25	0.7	6.083	+0.004	+0.008
0.30	0.7	7.270	−0.001	+0.001
0.35	1.0	8.457	0.000	0.000
0.40	1.0	9.610	−0.028	−0.030
0.45	0.7	10.845	+0.033	+0.030
0.50	1.0	11.988	+0.006	+0.006
0.55	0.7	13.139	−0.006	−0.002
			±0.012	±0.012

Meteor 689

t	$\sqrt{wt}$	s	$O-C$	$O-C$
s		km	km	km
0.00	0.6	0.000	+0.026	+0.020
0.05	0.6	3.027	−0.052	−0.044
0.10	1.0	6.171	+0.014	+0.019
0.15	1.0	9.182	−0.024	−0.029
0.20	1.0	12.262	+0.034	+0.027
0.25	0.6	15.180	−0.041	−0.023
			±0.029	±0.021

* Omitted in exponential solution.

strong evidence that the decelerations do not approximately follow the exponential law of increase with decreasing height in the atmosphere. The apparently constant decelerations observed by Millman and Miss Hoffleit and the erratically changing decelerations observed by Fedynsky and Stanjukovitch were probably produced by the method of reduction from limited material in the first instance and by the normal errors of measurement in the second. The values of the fractional decelerations ($-V_0'/V_0$) in Table II do not show a correlation with the velocity as suggested by Millman and Miss Hoffleit.

The successive residuals of Table I show some tendency to alternate in sign. Such an effect would indicate an eccentricity in the mechanical structure of the single-bladed shutter, opposite ends of the blade not being symmetrical

TABLE II

VELOCITIES AND DECELERATIONS

Equation	A	B	C	d	t_0	V_0	V_0'	V_0'/V_0
			Meteor 642					
	km	*km* *km*	*km* *km*		*s*	*km/sec*	*km/sec²*	
(1)	+0.0594	+31.3612 ±0.0626	−1.3590 ±0.0992		0.300	30.546	−2.718	−0.089
(2)	+0.0795	+31.0172 ±0.0382	−1.4770 ±0.1068		0.300	30.618	−2.659	−0.087
(3)	+0.1035	+31.1015 ±0.0425	−0.02031 ±0.00142	5	0.300	30.646	−2.276	−0.074
			Meteor 651					
(1)	+0.030	+37.235 ±0.216	−1.440 ±0.405	7	0.250	36.515	−2.88	−0.079
(3)	+0.070	+36.712 ±0.119	−0.0039 ±0.0019		0.250	36.556	−1.09	−0.030
			Meteor 660					
(1)	−0.0229	+13.9401 ±0.0304	−0.3396 ±0.0118		1.125	13.176	−0.679	−0.052
(2)	+0.0606	+13.5819 ±0.0335	−0.0890 ±0.0056		1.125	13.244	−0.601	−0.045
(3)	+0.1690	+13.6658 ±0.0491	−0.0886 ±0.0071	1.2	1.125	13.256	−0.492	−0.037
			Meteor 663					
(1)	+0.019	+79.881 ±0.089	−0.224 ±0.092		0.475	79.668	−0.448	−0.006
(3)	+0.049	+79.839 ±0.085	−0.0217 ±0.0107	2.3	0.475	79.690	−0.343	−0.004
			Meteor 670					
(1)	+0.034	+24.464 ±0.130	−1.138 ±0.210		0.275	23.838	−2.28	−0.096
(3)	+0.251	+24.777 ±0.194	−0.208 ±0.040	2.3	0.275	23.876	−2.07	−0.087
			Meteor 689					
(1)	−0.026	+62.384 ±0.578	−5.58 ±2.19		0.125	60.988	−11.17	−0.183
(3)	+0.010	+62.233 ±0.428	−0.0300 ±0.0095	10	0.125	61.188	−10.45	−0.171

about the axis of rotation. The chief result of eccentricity for the present investigation would be to increase the probable errors of both the velocities and accelerations. The probable errors of Table II, consequently, should be considered as

maximum values, the actual accuracy being somewhat greater than is indicated.

The values of V_0 and V_0' in Table II indicate that the calculated velocities of the meteors at the mid-points of the observed trails are almost independent of the law of deceleration assumed, and that the decelerations are only slightly dependent upon the law. Because an exponential law naturally follows from considerations of the structure of the atmosphere and the nature of the resistance, the values of V_0 and V_0' obtained from application of Equation (3) are used in all subsequent calculations involving these quantities.

The reduction of the observed velocity V_0 at the middle of the interval to the corresponding velocity, V_∞, if there had been no atmosphere (not including the so-called aberration effect of the earth's rotation and the attraction of the earth), was obtained by application of the following equation:

$$\log V_\infty = \log V_0 + \frac{9.0 \times 10^{11}}{V_0^2} \times \log\left(1 - 9.73 \times 10^{-7} \frac{V_0'}{\cos Z_R}\right), \quad (4)$$

where Z_R is the true zenith distance of the radiant and where c.g.s. units are used. The derivation of Equation (4) is given in Section 7 of this paper.

It should be particularly noted that an error in the time of a meteor's apparition affects the distance, s, along the trail by a constant factor only. The fractional deceleration and the form of the deceleration law are, therefore, completely independent of the time of observation. The correction to no atmosphere does, however, depend upon the time, but in no case is this correction large.

4. The Observational Results

The fundamental observational data and the resultant orbital elements for the six meteors are given in Table III.

TABLE III

FUNDAMENTAL DATA AND ORBITAL ELEMENTS

Meteor number:	642	651	660	660	663	663	670	689
AI plate number	32906	33020	33109		33120		33183	33416
FA Plate number	369	517	662		690		792	1171
Sidereal time (Oak Ridge)	$4^h\ 22^m\ 0^s$	$8^h\ 8^m\ 2^s$	$9^h\ 50^m\ 0^s$	$9^h\ 0^m\ 0^s$	$10^h\ 20^m\ 0^s$	$10^h\ 0^m\ 0^s$	$15^h\ 8^m\ 1^s$	$22^h\ 56^m\ 0^s$
U. T.	1936 Oct. 21.2972	1936 Dec. 14.3077	1937 Feb. 4.2364	1937 Feb. 4.2017	1937 Feb. 12.2353		1937 Mar. 12.3583	1937 Aug. 15.2565
Class	Sporadic	Geminid	Sporadic	——	Sporadic	——	Sporadic	Perseid
Length of trail (degrees)	6	7	20		43		8	15
Number of breaks measured	12	10	48		18		11	6
Length of time observed (sec)	0.60	0.50	2.25		0.95		0.55	0.25
Relative brightness of trail (AI)	Medium	Faint	V. faint		Faint		Faint	Medium
sin Q	+0.9998	+0.4815	+0.2286	——	+0.1109	——	+0.1594	+0.3365
cos Z_R	+0.8151	+0.9777	+0.5620	+0.6820	+0.1226	+0.0739	+0.4647	+0.7344
Height at first break (km)	89.69	92.82	66.54	73.81	119.53	101.90	76.27	96.97
Height at last break (km)	74.74	74.94	49.88	50.22	110.26	97.11	70.14	85.83
Height at middle (km)	82.08	83.69	57.97	61.68	114.90	99.50	73.15	91.34
Height at max. light (km)	77	84	53	55	114	99	70	91

TABLE III (*Continued*)

Meteor number:	642	651	660	660	663	663	670	689
Apparent radiant α (1900.0)	42° 59′.7	113° 14′.6	90° 9′.1	——	208° 4′.0	——	169° 41′.6	47° 46′.0
δ (1900.0)	+12 31 .9	+32 19 .9	+14 44 .2	——	−23 59 .7	——	+5 33 .8	+54 13 .8
Corrected radiant α (1900.0)	41 48	112 26	78 31	83° 33′	208 14	208° 23′	166 44	48 8
δ (1900.0)	+11 24	+32 2	+4 28	+8 45	−24 28	−24 38	+2 53	+54 24
Elongation (λ)	75 19	63 13	141 51	138 20	20 47	20 40	93 17	36 7
V_0 apparent rel. vel. (km/sec)	30.646	36.556	13.256	15.472	79.690	68.429	23.876	61.188
V_∞ no-atmosphere vel. (km/sec)	31.360	36.810	13.823	15.937	79.963	68.861	25.273	62.480
V_G Geocentric vel. (km/sec)	29.466	35.143	8.672	11.744	78.941	67.694	23.020	61.307
V_H Heliocentric vel. (km/sec)	36.285	34.531	37.397	39.740	51.859	40.884	38.814	41.366
a (a. u.)	1.910	1.457	2.222	4.062	−0.990	7.189	3.206	22.89
q (a. u.)	0.2962	0.1380	0.9684	0.9517	0.8342	0.7484	0.6178	0.9680
e	0.8449	0.9053	0.5641	0.7657	1.8423	0.8959	0.8073	0.9577
P (years)	2.639	1.759	3.311	8.186	——	19.275	5.740	109.51
Mean anomaly	−15° 35′	−20° 34′	−4° 13′	−2° 0′	— —	−1° 48′	−7° 16′	+0° 9′
ω (1900.0)	122 14	324 4	18 4	22 53	40° 28′	60 44	81 10	155 31
Ω (1900.0)	27 14	261 37	134 32	134 33	142 36	142 33	170 53	141 28
i (1900.0)	6 0	23 20	4 15	4 19	159 13	156 39	1 53	119 42

Notes on the Individual Meteors

Meteor 642.—The trail is typical, with a steady increase of intensity from the point of visibility to a maximum near the end where the intensity decreases rapidly. The point of maximum brightness could be measured with some accuracy, and there are three well-determined natural breaks near the end of the trail for apparition-time determinations. The maximum error in time should not exceed 1^m.

Meteor 651.—The trail is similar to that of No. 642, with very few irregularities. This Geminid was observed by Miss Dorrit Hoffleit and her associates during the shower. The apparent visual magnitude was estimated at -2^m. A luminous train could be seen for 20^s. The time error should not exceed 10^s.

Meteor 660.—This long and very faint trail showed few short natural breaks that could be measured accurately for time determinations. Superposition of the original plates permitted an uncertain matching of general features in the trails to yield a more probable sidereal time of about $9^h\,50^m$. Because of the uncertainty in time, the calculations in Table III were also carried out for sidereal time = $9^h\,00^m$. Almost the extreme range in solution is represented because the *FA* exposure ran from $8^h\,58^m$ to $9^h\,57^m$ Oak Ridge sidereal time. Forty-eight shutter breaks could be measured on the *AI* trail, but because of faintness the accuracy of measurement was low. The original x-measures, therefore, were fitted by a quadratic power series in the time, residuals plotted, and normal positions obtained at $0^s.25$ intervals. The s-values in Table I were obtained from these normal positions.

Meteor 663.—The natural irregularities in this trail were long and difficult to measure, although a number, photographic and natural, could be distinguished by careful study. A great amount of time was spent in matching these irregularities on the *AI* and *FA* trails but absolutely certain identifications could not be made. When repeated trials, with two different techniques in matching points, gave the same identifications of breaks, it seemed that the sidereal time of $10^h\,20^m$ was essentially correct. The eccentricity of the orbit, unfortunately, is very sensitive to changes in the apparent velocity arising from a change in the apparition time, and may vary from its minimum value of 0.9 (at sidereal time $10^h\,0^m$) to values much exceeding unity for later times. The physical data for this meteor point to the small eccentricity as correct. In the author's opinion the physical evidence outweighs the evidence from the uncertain matching of points; in other words, the meteor is probably of solar-system origin. The physical data are discussed in Section 9 of the present paper.

Meteor 670.—This meteor is distinguished by the occurrence of two bright flares in its trail. The apparition times calculated from measurements of the flares differed by only 7^s. The first flare occurred between the breaks at $t = 0^s.35$ and $t = 0^s.40$, and the second after the last break; the corresponding heights are 72.1 and 70.0 km respectively. No discontinuity in the motion after the first flare is noticeable in Table I. Probably a luminous train accompanied the flares, but its effect on the measurements of the breaks is not evident.

Meteor 689.—Four features in this Perseid trail could be very roughly measured for time determinations, with a resultant probable error of three or four minutes. The calculated time, however, was about two minutes after the end of the *FA* exposure. Since the meteor was not seen by the observer in closing the exposure, the time is assumed to be roughly two minutes before the end. The velocity, or height, is very little dependent upon the time in this case and a later time gives a smaller apparent velocity. Because of these fortuitous circumstances, the observed velocity cannot be appreciably in error from time inaccuracy and is a *minimum* value. This Perseid, then, serves as a check on rotating-shutter velocities.

For the two cases in which the time of apparition was uncertain two solutions each have been carried out; the first is the one that was considered more probable when the calculations were begun, while the other represents a limiting value in time and distance. Most of the table entries are self-explanatory. The number of breaks measured in the trail refers only to those measures that were used in the final reductions. The length of time observed is the length of time between the first and last breaks measured, or between the first and last normal places; the actual trail length usually represents a slightly longer interval. The angle Q is the angle between the poles of the great-circle motion as seen from the two stations; its sine represents the relative accuracy with which the apparent radiant is determined for equal precision in the determination of the great-circle motion. The length of the trail multiplied by sin Q is an approximation to the relative accuracy of the apparent radiant. It will be seen that all of the radiant determinations are of about the same accuracy—roughly two minutes of arc.

The heights given in Table III refer to the horizon plane passing through the AI camera at Oak Ridge; the zenith distances (Z_R) of the apparent radiant likewise refer to the zenith of the AI camera. The point of maximum light refers to the brightest point on the trail, including flares if any. The corrected radiant was obtained by correcting the apparent radiant successively for the so-called aberration effect of the earth's rotation and for the zenith attraction of the earth's gravitational field. The corresponding corrections to V_∞ gave V_G, the geocentric velocity. The meteor's height at the time of observation, as well as the earth's oblateness, were allowed for in the calculation of the earth's gravitational effect. It should be mentioned that the ordinary formula for gravitational acceleration at various latitudes does not apply in this case because the formula includes the centrifugal force of the earth's rotation.

A meteor's heliocentric velocity (V_H) is the vector sum of its geocentric velocity and the earth's heliocentric velocity.

The angle λ is the elongation of the corrected radiant from the apex of the earth's motion. In calculating the orbital elements no correction was applied for the small displacement in space of the meteor from its undisturbed path by the attraction of the earth.

5. Discussion of Astronomical Results

The only shower meteor of known velocity in Table III is No. 689. The radiant differs about three degrees from the radiant usually found for the Perseid shower,[12] but there is little doubt that the meteor is a member. Olivier [2] has found deviations of the same order of magnitude for Perseid meteors. The derived heliocentric velocity of 41.37 km/sec and consequent period of 110 years are in agreement with the 122-year period of Comet 1862 III, identified with the Perseid shower by Schiaparelli. Since the velocity of Meteor 689 is the least accurately determined of those in Table III (p.e. ± 0.6 km/sec) the agreement is better than would be expected. The facts that the velocity is a minimum value (in case of an error in the apparition time) and is near the parabolic limit are good evidence that meteor velocities determined by the rotating-shutter method are not subject to large errors produced by great deceleration in the upper atmosphere, as has been suggested by Millman and Miss Hoffleit. The correction for atmospheric deceleration, as given in Equation (4), is apparently of the right order of magnitude.

The other shower meteor in Table III is No. 651, a Geminid. The apparent radiant is in good agreement with the ephemeris determined observationally by Maltzev,[13] and indicates the greater accuracy of his ephemeris compared with that of King.[14] The radiant is also in fair agreement with determinations by Elkin and Olivier [2] in 1899. No large secular shift of the radiant is indicated. The small heliocentric velocity and consequent short period of 1.76 years for this Geminid cannot be questioned unless there is an error in

[12] Öpik, *H. C.* 388, 1934.
[13] *Russ. A. J.*, 8, 67, 1931.
[14] *M. N.*, 86, 638, 1926.

identification of the meteor by the visual observers. Such a resultant time error seems unlikely. It is possible that the orbit may have been seriously perturbed by a previous close approach to the earth and the period shortened, but the close agreement of the radiant with that of normal Geminids is a strong argument against the perturbation explanation. The short period and small perihelion distance are both consistent with the well-known facts that the Geminid shower contains a smaller proportion of faint to bright meteors than most of the well-recognized showers and is widely and uniformly distributed in space. If the short period is an indication of age, then the small members may have been largely removed from the principal orbit, radiation pressure being possibly the chief agent for removal. Such effects and the wide distribution of the stream members would be accentuated by a small perihelion distance. The Taurid shower is similar in nature to the Geminid shower, and very preliminary reductions of one Taurid trail, doubly photographed, indicate a short period. For full confirmation of these conclusions concerning the Geminid shower it is important that other members, now doubly photographed, be reduced. Maltzev favored a much longer period, corresponding to a semi-major axis of 13.7 a.u., from studies of the heights, visual estimates of velocity,and daily motion of the radiant. Elkin's best determination of a Geminid's apparent velocity agrees (to 0.4 km/sec) with the value obtained by the author.

Three of the four sporadic meteors studied have orbits only slightly inclined to the ecliptic and have short periods of revolution about the sun. The fourth has retrograde motion and an orbit of long period, nearly parabolic or possibly hyperbolic. It is clear that two entirely distinct types of orbit are exemplified; the first type, of small inclination and period, is similar to the orbits of asteroids with perihelion distance less than unity; the second type is definitely cometary in all respects. To show more concretely the similarity of the first type of orbit with the orbits of the inner asteroids and the short-period comets, comparative data are given in Table IV. The

TABLE IV

ASTEROIDAL METEORS (SPORADIC)

Identification	q	a	e	i
				°
No. 642	0.296	1.91	0.84	6.0
No. 660	0.968	2.22	0.56	4.3
No. 670	0.618	3.21	0.81	1.9
Mean	0.627	2.45	0.74	4.1
Asteroids ($q < 1$)				
Apollo *	0.647	1.48	0.56	6.4
Anteros †	0.441	1.86	0.76	1.4
1937 UB §	0.618	1.64	0.63	6.2
Mean	0.569	1.66	0.65	4.7
Comets ‡ ($P < 7$ yr. $q < 1$)				
Encke	0.332	2 21	0.85	12.6
Grigg-Skjellerup	0.908	2.93	0.69	17.4
Brorsen I	0.590	3.11	0.81	29.4
Giacobini-Zinner	1.000	3.52	0.72	30.7
Biela	0.856	3.52	0.76	12.6
Mean	0.737	3.06	0.77	20.5

* Whipple and Cunningham, *H. A.*, **105**, 537, 1937.
† Herget and Miss Davis, *H. A. C.* 366, Mar. 1936.
‡ A. S. Yamamoto, *Pub. Kwasan Obs.*, **1**, No. 4, 1936.
§ Cunningham, *H. A. C.* 440, Jan., 1938.

orbital elements q, a, e, and i are listed for the three asteroidal meteors, the three asteroids with perhelion distances less than unity, and the five comets of periods less than seven years and perihelion distances less than unity. Mean values of the orbital elements are calculated for each type of object. Since q, a, and e are related, and since the selection has been made according to q or q and a, it is clear that i is the only independent element for comparison purposes. The inclination, however, is not independent of selection effects, because there is a much greater probability of terrestrial encounter for meteors of small inclination; the probability of discovery is increased nearly as much for asteroids of small inclination because of their intrinsic faintness and rapidity of motion when bright

enough to be detected; similarly, there is a greater chance for the discovery of comets of small inclination, but this effect is not so marked as for the meteors and asteroids. In all three cases detection is more probable for small eccentricities.

With these facts in mind, the orbital similarities between the meteors and asteroids listed in Table IV still appear so marked that a distinction between the asteroidal meteors and the cometary meteors (including showers) seems justified. Any discussion of the evolutionary problems suggested is not warranted because of the limited observational material available. It is probable that a continuous sequence of types will be found when there are enough data for statistical discussion. Leuschner [15] has pointed out that such a continuum of orbital types exists for the asteroids and comets, and Bobrovnikoff [16] and others have discussed the evolutionary problems of the comets. Whether there exists an hyperbolic type of orbit among the bright meteors remains to be seen. Öpik [17, 18] and Boothroyd [19] have found evidence that a large proportion of the faint visual and telescopic meteors pursue hyperbolic orbits, but that the percentage of hyperbolic orbits decreases rapidly with increasing brightness and becomes very small for photographic meteors and fireballs. Wylie's [20] careful investigations of fireballs show that these bodies generally pursue orbits of short period. The radioactivity measures of the ages of meteorites by Paneth, Urry and Koeck [21] suggest strongly that fireballs originate in the solar system.

6. Magnitude Determinations

For a study of the physical processes of meteor phenomena it is important to know the intensity of the light emitted, and therefore to measure the apparent brightness. Since only one of the six meteors was observed visually, the brightness of

[15] *P. A. S. P.*, **39**, 275, 1927.
[16] *Lick Obs. Bull.* **14**, 28, 1929.
[17] *H. C.* 389, 1934.
[18] *H. C.* 391, 1934.
[19] *H. C.* 390, 1934.
[20] *Pop. Ast.*, **43**, 241, 312, 379, 603, 657, 1935; and **44**, 152, 1936.
[21] *Zeit. f. Phy. Chem.*, Abt. A, **1**, 152, 1931.

the others had to be obtained from the photographs. In no case were there star trails. It was necessary, therefore, to compare the meteor trails directly with the star images. Because of the faintness of the trails and the concentration of faint stars the comparisons were made visually. Star images of the same apparent density and width as the meteor trail at chosen points were selected, and star counts made on the original plate to these limits. Four areas of about 5.5 square degrees were counted for each magnitude limit. The van Rhijn tables [22] were used to determine the photographic magnitudes of the stars. On the same part of the plate several measures were made of the apparent star diameters, both across and along the images, in case of slight trailing caused by guiding errors. The mean value of the measured smaller diameter was assumed to be the diameter of an untrailed image. The effective exposure times for both the stars and the meteor were obtained from the ratios of this mean diameter to the length of the trail of known exposure time.

The effective exposure times varied from 4×10^{-4} sec for the fastest meteor to 8×10^{3} sec for the star images. The ratio thus reached the enormous value of 2×10^{7}. It seemed impossible that the failure of the reciprocity law for a photographic emulsion could be neglected. The facilities of the photographic department were not suitable for the determination of reciprocity failure over this range of exposure times, but through the kindness of Dr. C. E. K. Mees and Dr. Loyd A. Jones, two different emulsions were tested at the Eastman Kodak Laboratories. To conserve effort, measures were made only at photographic density 0.8 which is near that of most of the images observed and should give results of the correct order of magnitude at other density levels. The results were nearly identical for both the emulsions tested. The reciprocity-failure curve as determined by Dr. Loyd A. Jones for one emulsion is presented in Fig. 1. The circles represent the observed points. It can be seen from this figure that the photographic efficiency is actually very nearly the same for

[22] *Groningen Pub.* 43, 1929.

these extremes of exposure time, in keeping with the assumptions of zero reciprocity failure by Sytinskaya [23] and by Millman and Miss Hoffleit.[5] It is not safe, however, to generalize this result to other emulsions without actual measurements. Furthermore, if star trails were measured, the effective intensity might lie near the optimum intensity of the plate, so that much larger corrections than those applied here would be necessary.

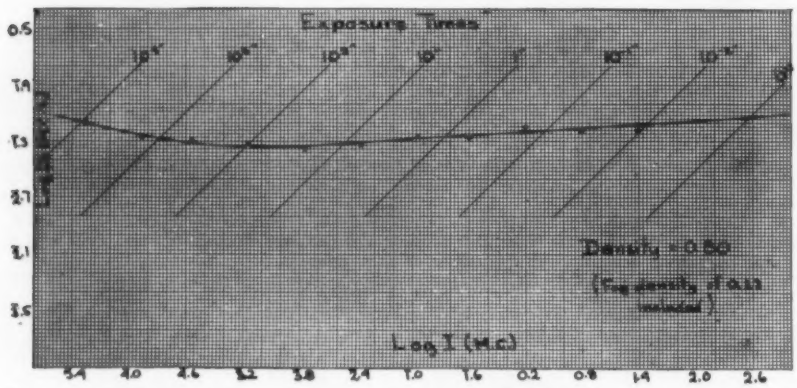

FIG. 1. Reciprocity-Law Failure. Cramer Hi-Speed Plate 29417[4]. Developed 10 min. at 68° F in 1 part rodinal + 32 parts water.

The relative exposure times of meteor images to star images of the same intensity, after correction for reciprocity failure by data of Fig. 1, represent the relative brightnesses. Data are given in Table V. Reductions of the derived meteor magnitudes to absolute magnitudes were made by allowing for the distance, no correction for atmospheric absorption being necessary since the stars studied were near the part of the trail under observation. An average difference of $0^m.37$ was found between the measures of the AI and FA plates; the systematic difference was $0^m.22$, the AI measures giving the greatest intensity. The average difference was reduced $0^m.12$, and the systematic difference $0^m.10$ by the correction for reciprocity failure. It is possible that some of the systematic difference may have arisen from the extreme faintness of the

[23] *Russ. A. J.*, **12**, 174, 1935.

TABLE V

MAGNITUDE DATA

Meteor:	642		651		660		663		670		689	
Sid. time:	$4^h\ 22^m$		$8^h\ 8^m$		$9^h\ 50^m$		$10^h\ 20^m$		$15^h\ 8^m$		$22^h\ 56^m$	
Camera:	FA	AI	FA	AI	FA	AI	FA	AI	FA	AI	FA	AI
Log n (av. light)	[1.12]	1.07	—	—	—	1.62	1.48	1.58	—	1.52	—	1.42
Log n (max. light)	0.77	0.91	0.82	1.09	1.01	1.04	—	—	0.53	0.70	1.36	1.26
Galactic longitude	121°	146°	175°	189°	73°	60°	137°	137°	127°	113°	23°	38°
Galactic latitude	−37	−27	+20	+34	+16	+30	+28	+44	+29	+28	−24	−42
Star mag. (av. light)	[11.68]	11.46	—	—	—	12.75	12.32	13.03	—	12.60	—	12.47
Star mag. (max. light)	10.75	11.00	10.24	11.40	10.90	11.12	—	—	10.03	10.37	11.74	12.03
App. vel. (deg./sec^{-1})	8.5	18.0	22.4	30.3	18.3	18.6	75.8	95.5	20.4	25.9	75.4	75.5
Log (eff. exp. star sec)	3.55	3.60	3.56	3.50	3.45	3.86	3.36	3.91	3.45	3.77	3.55	3.86
Log (eff. exp. meteor sec)	−2.41	−2.67	−2.48	−2.69	−2.72	−2.73	−3.38	−3.40	−2.81	−2.95	−3.43	−3.38
Corr. recp. fail. (mag.)	−0.22	−0.28	−0.25	−0.30	−0.35	−0.15	−0.50	−0.28	−0.35	−0.22	−0.48	−0.30
Corr. to dist. 100 km (mag.)	+0.14	+0.18	+0.10	−0.17	−0.36	−0.26	−0.68	−0.40	−1.00	−0.61	−0.10	−0.08
Abs. pg. mag. (av. light)	[−3.29]	−4.32	—	—	—	−4.14	−5.72	−5.93	—	−5.02	—	−6.01
Abs. pg. mag. (max. light)	−4.22	−4.78	−5.01	−4.55	−5.22	−5.77	—	—	−6.97	−7.25	−6.29	−6.45
Abs. vis. mag. (av. light)	−2.3		—		−2.2		−4.0		−3.1		−4.1	
Abs. vis. mag. (max. light)	−2.7		−3.0		−3.7		—		−5.3		−4.6	
Abs. vis. mag. (zero epoch)	−2.3		−2.6		−2.5		−4.0		−3.1		−4.1	

meteor trails on the FA plates. Comparable star images were, for some trails, below the magnitude limit of the plate (a continuous trail can be recognized when star images of the same brightness are too faint for certain identification). When this occurred, an attempt was made to allow for the effect by counting the uncertain grain aggregations in the emulsion, but it is possible that an overcorrection was made.

It is to be noted that a photographic meteor trail represents quite closely the intrinsic variations in the photographic luminosity of the meteor. For a constant linear velocity, the change in the angular velocity exactly compensates for the change in the square of the distance to the meteor.

The magnitude data of general interest are given in Table V. The entries are self-explanatory except for the quantity n, which represents the average number of stars per square degree counted on the plate. The absolute photographic magnitudes are those determined, in the manner explained above, at a point along the trail that appeared average in brightness and at the point of maximum light (including flares). A correction of $+ 1^m.8$ was applied to the absolute photographic magnitudes to obtain the absolute visual magnitudes. The value at the zero epoch was obtained by visual interpolation along the trail. The $1^m.8$ correction does not necessarily represent the color-index of a meteor, because the photographic magnitudes determined above may be subject to a considerable systematic error. Millman and Miss Hoffleit [5] found a systematic difference of this amount between the estimated visual magnitudes of four meteors and the photographic magnitudes determined in a similar fashion. Meteor 651 was observed as magnitude $- 2$ by Miss Hoffleit and her assistants; the average photographic magnitude calculated for Oak Ridge would be about $- 4^m.2$, a difference of $2^m.2$. Similarly, a study of the frequency of meteors by Watson [24] indicates that the visual-magnitude limit of the AI camera is between 0 and $- 1$, suggesting a correction even larger than that applied. A rough study of the meteors seen

[24] *H. A.*, 105, 623, 1937.

visually but not registered by the cameras, on the other hand, suggests that this limit is too faint. For example, Mr. Cunningham informed me that he had observed a slow-moving meteor, brighter than Sirius, that passed over the field of the FA camera. A careful search on both the FA and AI plates taken at that time failed to reveal a meteor trail. Several similar cases have occurred in recent months since the observers at Cambridge and Oak Ridge have been kindly recording and transmitting to me their visual observations of meteors. No photographic meteors have, so far, been recorded by these observers. It is probable that the visual limit of the AI and FA cameras is brighter than magnitude − 1.

There is some direct evidence that meteors may have a negative color index. Images of meteor flares near the edges of the fields of FA and AI plates show much greater effects of coma and astigmatism than do star images of comparable central intensity. This is due, presumably, to a strong concentration of ultra-violet light and to lack of achromatism in the ultra-violet, since the sensitivity of the Cramer Hi-Speed plates cuts off sharply in the blue-green. Unquestionable evidence of intense ultra-violet light is shown in the spectra of meteors as discussed by Millman.[25] A recent spectrum [26] shows the effect strikingly. By a crude reduction of Millman's published data the author derived a negative color index for the flare spectrum of Meteor XXIV.[27]

Although the visual magnitudes in Table V are not obtained with precision, they represent the results of a self-consistent and repeatable method. The author plans to determine the errors in this type of magnitude determination from direct observations in the future. Besides a possible systematic error of appreciable size there may be errors varying with the magnitude and velocity of the meteor. It is believed, however, that the visual magnitudes of Table V are of the correct order.

[25] *H. A.*, **82**, 149, 1935.
[26] P. M. Millman, *Jour. R. A. S. C.*, **30**, 25, 1936.
[27] *H. A.*, **82**, 155, 1935.

7. *Physical Theory*

Theories for the physical processes undergone by a meteoroid in the earth's atmosphere have been developed by various investigators, in particular by Öpik,[28] Lindemann and Dobson,[29] Hoffmeister,[30] Sparrow,[31] Maris,[32] Fisher,[33] Wylie,[34] and Hoppe.[35] In the present paper the notation and principles of Hoppe's dissertation will be closely followed and use made of Öpik's calculations of the luminous efficiency. The data that are assumed for a given meteor are V_0, V'_0, H_0, and I_0, its velocity, acceleration, height, and "visual" intensity (erg/sec), respectively, at the middle of the observed trail, and V_m, H_m, and I_m at the point of the trail where maximum luminosity is attained. A solution is made to determine the fraction of the relative kinetic energy of the air molecules available for heating the nucleus, and for the density of the atmosphere at the midpoints of the respective trails. The masses and radii of the meteoroids are determined, and formulæ developed for calculations with incomplete data.

(A) DECELERATION AND MASS LOSS

In its passage through air of density ρ a meteoroid of mass m will, in time dt, encounter an air mass

$$dm_a = A' m^{2/3} \rho V \, dt, \tag{5}$$

where the value of A' depends upon the shape and density of the meteoroid and is $(9\pi/16\rho'^2)^{1/3}$ for a sphere. The total energy lost by the meteoroid is $d(1/2mV^2)$, if we assume that its original internal energy is negligible. The loss of energy through loss of mass dm will not serve to decelerate the remaining nucleus and may be neglected in calculating atmos-

[28] (*a*) *Veröff. Dorpat*, **25**, 1, 32 (1922); (*b*) H. R. 100, 1933; and (*c*) *Pub. Obs. Ast. Tartu*, **29**, No. 5, 1937.
[29] *Proc. Roy. Soc.* A, **102**, 411, 1923.
[30] *A. N.*, **221**, 353, 1924.
[31] *Ap. J.*, **63**, 90, 1926.
[32] *Terr. Magn.*, **34**, 309, 1929.
[33] *H. C.* 385, 1934.
[34] *Pop. Ast.*, **44**, 281, 1936.
[35] *A. N.*, **262**, 169, 1937.

pheric deceleration. The energy lost from deceleration will be transferred to kinetic energy, dB, of the air particles and to heat energy, dW, in the nucleus. This relation may be stated as follows:

$$-mV\,dV = \gamma V^2\,dm_a = dB + dW$$
$$= 1/2\overline{\eta^2}V^2\,dm_a + 1/2\bar{\lambda}V^2\,dm_a, \quad (6)$$

in which the constants $\overline{\eta^2}$, $\bar{\lambda}$, and γ are defined by the equations, the $\overline{\eta^2}$ term equals dB, and the $\bar{\lambda}$ term equals dW. To show the significance of $\overline{\eta^2}$ and γ it is noted that if the air particles (at rest) collide elastically with the nucleus, receiving velocities V_a at angles φ to the direction of the motion, then

$$\overline{\eta^2}V^2 = (\overline{V_a^2}), \quad (7a)$$

$$\gamma V = \overline{V_a \cos\varphi}, \quad (7b)$$

and

$$\bar{\lambda}V = 2\,\overline{V_a \cos\varphi} - (\overline{V_a^2})/V, \quad (7c)$$

where the bars represent values averaged over all solid angles. Equation (7a) arises from the conservation of energy, (7b) from the conservation of momentum, and (7c) from Equation (6). If the air particles rebound elastically with particles of the meteoroid slightly detached from the surface, the heating effect being transmitted to the nucleus by the repercussion of meteoric particles alone, the values of $\overline{\eta^2}$, γ, and $\bar{\lambda}$ can be calculated. For different mass ratios of spherical meteoric particles to spherical air particles, these values are given in Table VI.

TABLE VI

Mass ratio	γ	$\overline{\eta^2}$	$\bar{\lambda}$	$\bar{\lambda}/\gamma$
∞	0.67	1.33	0.00	0.00
9	0.60	1.08	0.12	0.20
4	0.53	0.85	0.21	0.40
3	0.50	0.75	0.25	0.50
2	0.44	0.59	0.30	0.67
1	0.33	0.33	0.33	1.00
*	1.00	1.00	1.00	1.00

* $\overline{V_a \cos\varphi}/V = (\overline{V_a^2})/V^2 = 1$.

The mass ratio ∞ corresponds to a solid spherical meteoroid striking air particles elastically. There is no energy for heating the nucleus, hence $\bar{\lambda} = 0$. The mass ratio of 2 corresponds roughly to iron atoms striking nitrogen molecules. The case $\overline{V_a \cos \varphi}/V = (\overline{V_a^2})/V^2 = 1$ is that in which the air particles are carried along with the meteoroid by the "penetration trap" discussed by Öpik.[28c] Here the value $\bar{\lambda} = 1$ represents the heat energy available for vaporizing the material of the nucleus and is an upper limit to the amount that actually does so. In the following discussions of this paper the symbol $\bar{\lambda}$ will be used in the latter sense, *i.e.*, to represent the heat energy that is actually used in vaporizing the nucleus. It will be noted that Equation (6) will be inaccurate, but since the $\overline{\eta^2}$ term is not used in the subsequent theory, the remaining heat energy can be considered as added to this term.

Equations (6) and (5) lead to the deceleration equation:

$$V' = dV/dt = -\gamma A' m^{-1/3} \rho V^2. \tag{8}$$

When the atmospheric density, the velocity, and the deceleration are known, this equation provides a solution for the meteoric mass:

$$m_0^{1/3} = -\gamma A' \rho_0 V_0^2 / V_0'. \tag{9}$$

If all of the heat dW (or the $\bar{\lambda}$ energy, with the convention mentioned above) is utilized in vaporizing the nucleus, and if the vaporized material is quickly brushed away by the pressure of the air, the rate of loss of mass is obtained from Equations (6) and (5):

$$\frac{dm}{dt} = -\frac{1}{\zeta}\frac{dW}{dt} = -\frac{\bar{\lambda}}{2\zeta} A' m^{2/3} \rho V^3, \tag{10}$$

where ζ is the latent heat of vaporization for the meteoric material. The latent heat of fusion is relatively small and is not included.

In the ratio of Equations (10) to (8), the time and atmospheric density are eliminated, so that an integration gives the

following relation between the mass and velocity:

$$ln\left(\frac{m}{m_\infty}\right) = \frac{\bar{\lambda}}{4\gamma\zeta}(V^2 - V_\infty^2), \tag{11}$$

where the subscript ∞ refers to the values of the quantities outside the influence of the earth's atmosphere. This equation is less useful than it would appear to be because the height and atmospheric density enter in a complicated manner when the other integrals are obtained. The complete solution for velocity and mass as a function of height has been obtained by Hoppe in terms of the exponential integral. For the problems of the present paper this form is difficult and a convenient approximation can be used because the change of velocity is actually small compared to the change of mass over the observed path.

Since the mass at the observed epoch is given by Equation (9), the mass outside the earth's atmosphere can be obtained by an integration of (10) in which the variation of V is neglected. Changing from the variable time to the distance along the path, s, we find the integral of (10) to be

$$m^{1/3} = m_\infty^{1/3} - \frac{\bar{\lambda}A'}{6\zeta} V^2 \int_\infty^s \rho \, ds. \tag{12}$$

If an isothermal homogeneous atmosphere exists above height H_0, the density is given by

$$\rho = ae^{-bH}, \tag{13}$$

where a and b are constants. The differential ds becomes

$$ds = -d\rho/(b\rho \cos Z_R), \tag{14}$$

where Z_R is the true zenith distance of the radiant. Equation (12) then integrates to

$$m^{1/3} = m_\infty^{1/3} - B'\rho, \tag{15}$$

or

$$m_\infty^{1/3} = m_0^{1/3} + B'\rho_0,$$

in which

$$B' = \bar{\lambda}A'V_0^2/(6b\zeta \cos Z_R). \tag{15a}$$

The correction of V_0, observed, to V_∞ is obtained from Equations (15a), (11), and (9) in the form

$$V_\infty^2 = V_0^2 + \frac{12\gamma\zeta}{\bar{\lambda}} ln \left(1 - \frac{\bar{\lambda} V_0'}{6b\gamma\,\zeta \cos Z_R} \right) . \quad (16a)$$

An alternate form of this relation may be obtained by substituting Equation (15) in (8) and integrating with respect to ρ. The velocity correction equation becomes

$$\log \left(\frac{V_\infty}{V_0} \right) = \frac{6\gamma\zeta}{\bar{\lambda} V_0^2} \log \left(1 - \frac{\bar{\lambda} V_0'}{6b\gamma\,\zeta \cos Z_R} \right) . \quad (16b)$$

When Equation (16a) or (16b) is expanded in series form and higher order terms neglected, it reduces to

$$V_\infty = V_0 \left(1 - \frac{V_0'}{V_0^2 b \cos Z_R} \right) . \quad (16c)$$

Of these three equations, (16a) is preferable and will be used in all subsequent work. Actually (16b) was used in the present investigation and is written in numerical form as Equation (4). A very good approximation, however, is given by (16c) where the physical constants $\bar{\lambda}$, γ, and ζ cancel out. In a heterothermal atmosphere the value of b (or $\rho^{-1} d\rho/dH$), that applies to the height at which V_0 is determined, should be used.

(B) THE LIGHT INTENSITY RELATION

In Hoppe's theory the light intensity of the meteor is assumed to be proportional to the energy lost through loss of matter $\left(1/2 V^2 \frac{dm}{dt} \right)$, plus the energy, dB, lost in direct collisions with the air particles. The light is thus produced in a coma about and behind the meteoroid, in keeping with the theory developed by Öpik for bright visual meteors. The intensity relation is written:

$$I = \tau \left(\frac{dB}{dt} + \frac{1}{2\zeta} V^2 \frac{dW}{dt} \right) = \frac{1}{2} \tau A' \left(\bar{\eta^2} + \frac{\bar{\lambda} V^2}{2\zeta} \right) m^{2/3} \rho V^3 . \quad (17)$$

From Öpik's [28b] calculations for bright visual meteors the efficiency factor is quite closely

$$\tau = \tau_0 V, \tag{18}$$

in which $\log \tau_0 = -9.07$. In practice the constants of Equation (17) are such that the $\overline{\eta^2}$ term is relatively small, *i.e.*, the energy from impact of the vaporized material of the meteoroid with the air is very much greater than the energy from direct impact of the nucleus with the air. Equation (17) then may be written:

$$I = \frac{\tau_0 \bar{\lambda} A'}{4\zeta} m^{2/3} \rho V^6, \tag{19}$$

in which I is now the "visual intensity" defined by Öpik.[36]

In photographic meteor trails the point of maximum light is perhaps more determinate and less ambiguous than the beginning and end points. It is clear that theoretically the height of the point of maximum light depends upon the constants adopted in the equations so far derived. The value of $\bar{\lambda}/\gamma$ for an individual meteor can be calculated in terms of the observed data. The condition for light maximum from Equation (19) becomes

$$\frac{2}{3m}\frac{dm}{dt} + \frac{1}{\rho}\frac{d\rho}{dt} + \frac{6}{V}\frac{dV}{dt} = 0. \tag{20}$$

A substitution of the values of the derivatives from Equations (10), (14), and (8) leads to the following equation applying at maximum light:

$$A'\rho_m = \frac{3b\zeta \cos Z_R}{\bar{\lambda} V_m^2 + 18\gamma\zeta} m_m^{1/3}, \tag{21}$$

where an isothermal atmosphere is assumed and where the efficiency factor enters only in the number eighteen. When the value of m_m is obtained from Equation (15), m_∞ from (15*a*), and m_0 from (9), the equation becomes

$$\bar{\lambda}/\gamma = \left[\frac{b \cos Z_R}{(-V_0')} - \frac{6\rho_m}{V_0^2 \rho_0}\right]\left(\frac{6\zeta\rho_0}{3\rho_m - \rho_0}\right), \tag{22}$$

[36] *Pub. Obs. Ast. Tartu*, **29**, No. 5, p. 52, 1937.

in which V has been assumed constant over the interval. The solution is largely dependent upon V_0' and less so upon the atmospheric densities, since maximum light usually occurs near the height at which V_0 and V_0' have been determined. Approximate values for the atmospheric density are thus sufficient for the determination of $\bar{\lambda}/\gamma$; the numerical value of the intensity (which is relatively uncertain) does not enter.

(c) EQUATIONS FOR ATMOSPHERIC DENSITY OR FOR VELOCITY IN SPECIAL CASES

When complete observations are available for a meteor, the atmospheric density can be determined as a function of the efficiency factor and the known data. Although the efficiency factor is somewhat uncertain as yet, it enters the equation only to the one-third power. Equation (19) is used at the zero epoch and m_0 obtained from Equation (9). The resultant equation for density is

$$\rho_0 = (V_0)^{-10/3}(V_0')^{2/3}(I_0)^{1/3}(\tau_0)^{-1/3}K, \tag{23}$$

where $K^3 = 4\zeta/(\bar{\lambda}\gamma^2 A'^3)$. This equation is especially useful, as the exponents of the quantities decrease with decreasing accuracy in measurement. Thus the density is surprisingly well determined.

Where the deceleration is not observed, the maximum-light relations can be used in the determination of density, or if both deceleration and velocity are unknown the velocity can be determined when the density is assumed. For this purpose Equation (21) is used and m_m determined from the mass-time derivatives

$$\frac{dm}{dt} = -\frac{2I}{\tau V^2} = -\frac{\bar{\lambda}A'}{2\zeta}\, m^{2/3}\rho V^3; \tag{24}$$

solving for m

$$m^{2/3} = 4I\zeta/(\tau_0\bar{\lambda}A'\rho V^6), \tag{25}$$

and the density equation becomes

$$\rho_m = V_m^{-10/3}(I_m)^{1/3}(b\cos Z_R)^{2/3}LN, \tag{26}$$

where

$$L = \zeta 6^{2/3}/(\bar{\lambda}A'\tau_0^{1/3}), \quad \text{and} \quad N^{-3/2} = 1 + 18\gamma\zeta/(\bar{\lambda}V_m^2) \cong 1.$$

Equation (26) can be used for the determination of ρ_m when V_m is known, or for V_m with an assumed ρ_m. It is especially valuable in the latter instance because good observations made photographically or visually from two stations can be used for an approximate determination of the velocity, if the height of maximum light and the magnitude are observed.

Another case may arise in two-camera photographic work when the shutter speed is unknown or uncertain; V_0'/V_0 can then be obtained from the observations, but V_0 is unknown. Equation (23) provides the solution for V_0 with an accuracy of $(\rho_0)^{3/8}$; the errors in I_0 and τ_0 introduce an almost negligible effect.

When a meteor is photographed with a single camera equipped with a rotating shutter, Millman and Hoffleit[5] have shown that the radiant can be determined with a satisfactory degree of accuracy. The deceleration would usually be indeterminate from such limited data. In this case, however, the following relations hold:

$$V = vR, \tag{27}$$

$$I = iR^2, \tag{28}$$

and

$$H = hR, \tag{29}$$

where R is the unknown distance from the station to a point on the trail, and v, i, and h are constants that can be determined from the data. When Equations (27) and (28) are substituted in Equation (26), we have, at the point of maximum light:

$$\rho_m = (R_m)^{-8/3}(v_m)^{-10/3}(i_m)^{1/3}(b \cos Z_R)^{2/3}L, \tag{30}$$

where N is neglected for the first approximations. Trial values of R can be substituted in (29) and (30) to determine H_m and ρ_m. The height-density relationship, assumed to be known, provides a second value of ρ_m. By successive trials R_m is determined and the solution is complete. If V_0'/V_0 can be determined, Equation (23) can be used in the same manner as was (26), to provide a much more reliable solution for the velocity.

8. *Numerical Values of the Constants*

(A) ATMOSPHERIC COMPOSITION AND DENSITY

At the present time there is practically universal agreement that the composition of the upper atmosphere is not predominately hydrogen as was once maintained by Humphreys [37] and others. The arguments in favor of a nearly homogeneous composition up to 100 km or more need not be repeated here. Reference may be made to Chapman,[38] Chapman and Milne,[39] Öpik,[28c] and others. We shall assume a uniform composition of the atmosphere up to the heights of the meteors observed photographically. On this assumption and that of a mean molecular weight of 28.8,

$$b = 3.43 \times 10^{-4}\, T^{-1}, \tag{31}$$

where T is the absolute temperature, and c.g.s. units are used as in all of the physical theory. In obtaining the numerical Equation (4) from Equation (16b), the value $b = 1.14 \times 10^{-6}$ was used. This value corresponds to an atmospheric temperature of 300° K. The accuracy of Equation (4) might have been somewhat improved by adopting the correct value of b (or $\rho^{-1}d\rho/dH$) at the observed heights, except for the fact that the actual values are still uncertain. The temperatures at the heights of the observed meteors are usually less than 300° K, so that Equation (4), as it stands, represents an over correction. In applying Equation (22) for the determination of $\bar{\lambda}/\gamma$ below, b is assumed to be 1.5×10^{-6} (corresponding to $T = 228°$), not far from the most probable mean value for the heights involved.

In an atmosphere of varying temperature under a constant gravitational field, the density is obtained from the integral

$$\log \rho = 4.96 \times 10^{-7} \int_{H}^{2\times10^{6}\ \mathrm{cm}} \frac{300}{T}\, dH + \log \frac{300}{T} - 4.20. \tag{32}$$

In this equation the fundamental numerical data are the ob-

[37] *Physics of the Air*, 1929.
[38] *Proc. Roy. Soc.* A, **132**, 353, 1931.
[39] *Quart. J. Roy. Met. Soc.*, **46**, 357, 1920.

servations made during the flight of the "Explorer II"[40] that the air pressure is 41.0 mm of Hg and the temperature − 58° C, at an altitude of 20 km above sea level. The value of a is not essential in the theory since the density given by Equation (32), or obtained by other means, is usually required.

(B) THE METEOROID

The nature of the meteoric material enters the equations only through the quantity A' and the latent heat of the vaporization. The value $\zeta = 6 \times 10^{10}$ ergs gm^{-1} is quite closely correct for both iron and stone meteorites[28c] and will be adopted as final. The quantity A' represents the cross-sectional area of the meteoroid perpendicular to the line of motion, divided by the total mass to the two-thirds power. For a spherical iron with $\rho' = 7.8$ the value of A' is 0.31, and for a stone with $\rho' = 3.4$ the value is 0.53. For a slowly rotating brick-shaped stone of dimensions $2 \times 3 \times 6$ the value of A' is 0.73 and for an iron, 0.42. If the meteoroid is elongated in the direction of motion and does not rotate about a minor axis, the value of A' may become quite small. Since there is no obvious method for distinguishing stones from irons among the meteors actually observed, we adopt the value $A' = 0.5$, corresponding to a slightly elongated stone that does not rotate or to an iron somewhat flattened in the direction of motion.

(C) THE CONSTANTS $\bar{\lambda}$ AND γ

The only empirical determination of $\bar{\lambda}$ from meteoric data is that by Hoppe. He used Rosenhagen's[41] calculations of the deceleration and the loss of mass of the Prambachkirchen meteor to determine $\bar{\lambda}$. His result was $\bar{\lambda} = 0.023$. This value is much less than the theoretical values obtained by Sparrow[31] and Öpik,[28c] and the values calculated in Table VI for elastic collisions. It is highly probable that a "penetration trap" of the type described by Öpik is the actual process

[40] *Nat. Geographic Soc.*, "Stratosphere Flight of 'Explorer II,'" 1936.

[41] *Mitt. Wiener Sternw. No. 4*, p. 245, 1935; J. Schadler and J. Rosenhagen, *Jahrb. d. Oberösterr.*, Musealvereines, **87**, 1935.

by which the air particles react with the gaseous atmosphere of the meteoroid. In this process the air particles are trapped in the moving atmosphere by multiple collisions and are momentarily carried along with the meteoroid. One-half of the energy lost by the nucleus is imparted to the air molecules directly, and the other half is converted into heat. Of this heat, probably a large percentage is taken up in fusing and vaporizing the nuclear material. Öpik places this fraction at the high value of from 60 to 80 per cent.

In the "penetration trap" process $\gamma = 1$. It is possible to derive $\bar{\lambda}/\gamma$ directly from the present observations by application of Equation (22). Numerical values for ρ_0 and ρ_m were taken from Pekeris' [9] calculations of atmospheric temperatures. The resulting values of $\bar{\lambda}/\gamma$ are presented in Table VI. Because of the considerable scatter, means were taken in various ways. The straight mean is 0.73, the harmonic mean 0.32, and the logarithmic mean 0.46. We adopt $\gamma = 1.00$ and $\bar{\lambda} = 0.50$ for all subsequent calculations, although the value $\bar{\lambda}/\gamma = 0.4$ was used in the calculation of Equation (4). The very low values, $\bar{\lambda} = 0.05$ and $\bar{\lambda} = 0.005$, used by Hoppe in his numerical calculations, lead to improbable conclusions concerning the quantitative behavior of meteors in the atmosphere and, to some extent, concerning the qualitative behavior.

The close agreement, in the mean, of the observed values of $\bar{\lambda}/\gamma$ with the theoretical values of Sparrow and Öpik, and with the laboratory values,[28c] shows clearly that the physical theory adopted must be essentially correct. It should be noted particularly that equation (22) is based not only upon the deceleration and light-intensity equations, but upon the maximal condition for the light equation. The scatter in the calculated values of $\bar{\lambda}/\gamma$ is not surprising when one considers the many indeterminate quantities that enter, the large possible variations in the shape and composition of the meteoroids, the idealized nature of the physical processes, and the lack of detailed knowledge concerning atmospheric conditions at the observed heights.

(D) VISUAL EFFICIENCY AND INTENSITY

The quantity I has been defined by Öpik [42] as the "visual intensity." He connects it with the visual absolute magnitude M_v of a meteor by the relation

$$\log I = 9.84 - 0.4\, M_v, \tag{33}$$

where I is measured in ergs sec^{-1} over the spectrum interval from λ4500 to λ5700. The author does not duplicate the constant 9.84 closely but it will be used in the subsequent calculations because Equation (33) should be consistent with Öpik's [43] calculations for the visual efficiency of radiation, τ (in his notation, β). The form and numerical constant for this quantity are given in Equation (18) above.

9. The Density of the Atmosphere and other Physical Results

The observational data of physical interest are presented in Table VII. The bracketed values represent the less probable solution for the meteor in question and are omitted

TABLE VII

PHYSICAL RESULTS

Meteor no.:	642	651	660	660	663	663	670	689
Date:	Oct. 21	Dec. 14	Feb. 4		Feb. 12		Mar. 12	Aug. 15
Sidereal time:	$4^h\,22^m$	$8^h\,8^m$	$9^h\,50^m$	$9^h\,0^m$	$10^h\,20^m$	$10^h\,0^m$	$15^h\,8^m$	$22^h\,56^m$
H_0 (cm × 10^{-5})	82.4	84.3	58.9	62.9	115.1	99.7	74.4	91.8
H_m (cm × 10^{-5})	77	84	54	56	114	99	71	91
V_0 (cm × 10^{-5})	30.65	36.56	13.26	15.47	79.69	68.43	23.88	61.19
$-V_0'$ (cm × 10^{-5})	2.28	1.09	0.492	0.574	0.34	0.30	2.07	10.4
p.e. V_0' (cm × 10^{-5})	±0.16	±0.53	±0.039	±0.46	±0.17	±0.15	±0.40	±3.3
$\log I_0$	10.76	10.88	10.84	10.96	11.44	11.32	11.08	11.48
$\log I_m$	10.92	11.04	11.32	11.44	11.44	11.32	11.96	11.68
$\bar{\lambda}/\gamma$	0.28	2.41	0.84	[0.79]	[0.74]	0.48	0.24	0.14
$\log \rho_0$	−7.24	−7.67	−6.45	[−6.59]	[8.95]	−8.81	−6.80	−7.57
$\Delta \log \rho_0$	+0.02	+0.08	0.00	—	—	−0.02	−0.09	−0.22
m_0 (gm)	1.7	2.3	253	[154]	[1.1]	1.8	10.4	0.1
m_∞ (gm)	3.3	3.0	320	[192]	[2.3]	4.8	29.3	1.4
r_∞ (cm)	0.62	0.60	2.82	[2.38]	[0.54]	0.69	1.26	0.46
$\log \rho_m$	−7.12	−7.12	−6.09	[6.19]	[−8.77]	−8.72	−6.78	−7.74
$\Delta \log \rho_m$	+0.27	−0.44	−0.14	—	—	−0.06	−0.01	+0.02

[42] *Cf.* Ref. 28*c*, p. 52.

[43] *Cf.* Ref. 28*b*, Table XII, p. 35.

when means are made. The heights given in this table have been corrected for the curvature of the earth and for the altitude of the station above sea level. They are, therefore, heights above sea level. The logarithms of the atmospheric densities at the zero epochs (log ρ_0) were calculated by means of Equation (23) where log $K = 7.22$. The values are indicated by crosses in Fig. 2; a dashed line connects two solutions

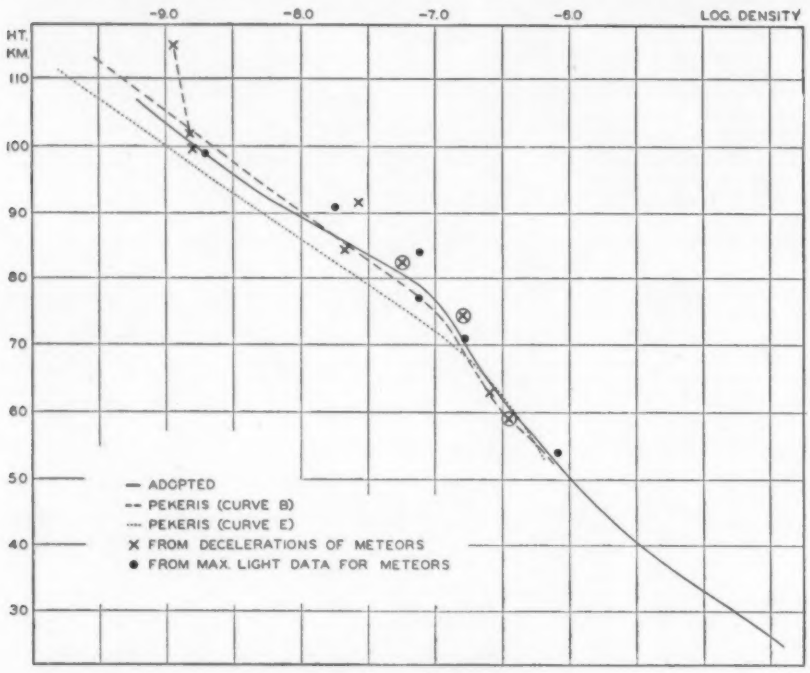

FIG. 2. Atmospheric density.

for the same meteor. The logarithms of atmospheric densities at the times of maximum light were calculated by means of Equation (26) in which log $L = 14.92$ and in which b, $(\rho^{-1}d\rho/dH)$, was obtained from Table VIII, to be described below. The densities at maximum light are indicated by dots in Fig. 2. The values of log I_0 and log I_m were obtained from Table V by means of Equation (33), and the values of m_0 and m_∞ were calculated from Equations (9) and (15a), respectively. The radii of the meteors before they penetrated the earth's

TABLE VIII

ADOPTED ATMOSPHERIC CONDITIONS

H	T	$-\log \rho$	$-\frac{1}{\rho}\frac{d\rho}{dH} \times 10^6$	H	T	$-\log \rho$	$-\frac{1}{\rho}\frac{d\rho}{dH} \times 10^6$
20 km	219°	4.06	1.56	70 km	309°	6.78	0.67
25	220	4.40	1.56	75	230	6.93	0.89
30	223	4.74	1.62	80	184	7.20	1.64
35	256	5.12	1.79	85	186	7.62	1.99
40	299	5.45	1.36	90	206	8.04	1.91
45	322	5.73	1.19	95	233	8.44	1.74
50	340	5.97	1.11	100	266	8.79	1.55
55	356	6.21	1.04	105	303	9.11	1.39
60	365	6.42	0.94	110	347	9.46	1.24
65	355	6.62	0.82				

atmosphere, r_∞, were calculated from m_∞ for spheres of density 3.4. The values of r_∞ would be appropriately reduced were a greater density assumed. For the sake of visual representation, the cross-sections of the meteors, derived in this way, are drawn in Fig. 4.

In Fig. 2, density curves have been drawn to represent the calculations by Pekeris.[9] The general agreement of the meteor determinations with his curve (B) is satisfactory except for the hyperbolic solution in the case of Meteor 663. After a considerable study of the problem of densities at this altitude it seems probable that the hyperbolic solution requires too high a density and that an elliptic solution (lying between the two adjacent crosses near $H = 100$ km) should be adopted. For Meteor 660 the two calculated solutions define a line that is nearly parallel to the theoretical density curve; thus, no choice in solution is indicated. The only marked discrepancy among the data in Fig. 2 occurs for Meteor 689 (at $H = 91.8$ km). Although this may actually be a seasonal effect (see below), other evidence indicates that the deceleration may be in error. The observational material is scanty, the calculated value of $\bar{\lambda}/\gamma$ suspiciously small, and the calculated ratio of m_0 to m_∞ impossibly small. For these reasons the density calculated for this meteor should be given a low weight. In weighing the other density determinations it should be noted

that the decelerations measured for Meteors 642, 660, and 670 have relatively small probable errors. The corresponding densities at heights 82, 59, and 74 km, respectively, should be

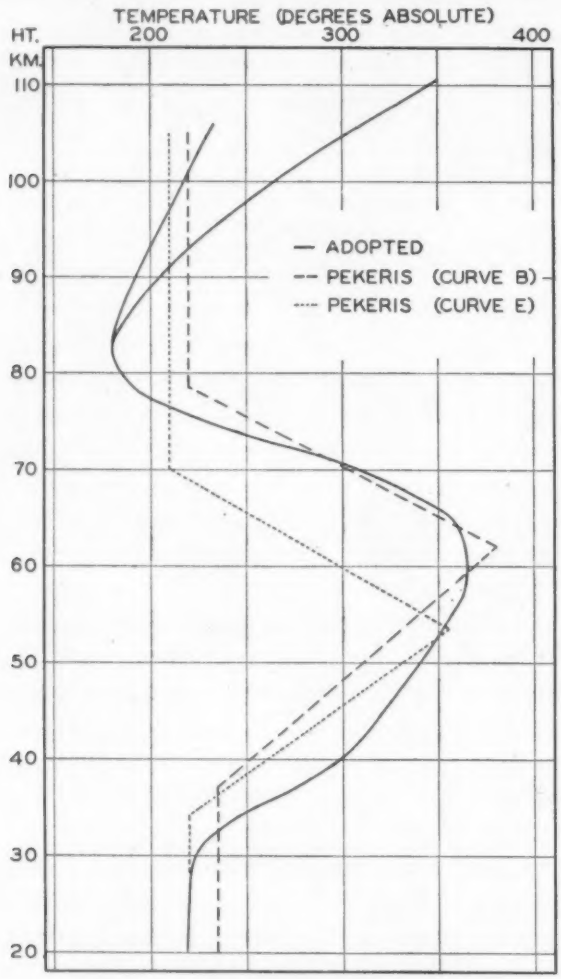

FIG. 3. Atmospheric temperature.

considered the most accurate; these values are indicated by circles about the crosses in Fig. 2. For Meteor 651 ($H = 84.3$ km), the deceleration is of low weight and the calculated value of $\bar{\lambda}/\gamma$ is too large.

With the above limitations of the data in mind, we see that the calculated densities are consistent with the general features of atmospheric structure that are now recognized, a rising temperature above the high stratosphere, a maximum in the neighborhood of 60 km, and a lower temperature above this level. A subsequent rise in temperature at levels of the order of 100 km and above cannot be ascertained from the present data. The "adopted" density curve of Fig. 2 was obtained from the "adopted" temperature curve of Table VIII and Fig. 3 by means of the integral (32). As stated above, a pres-

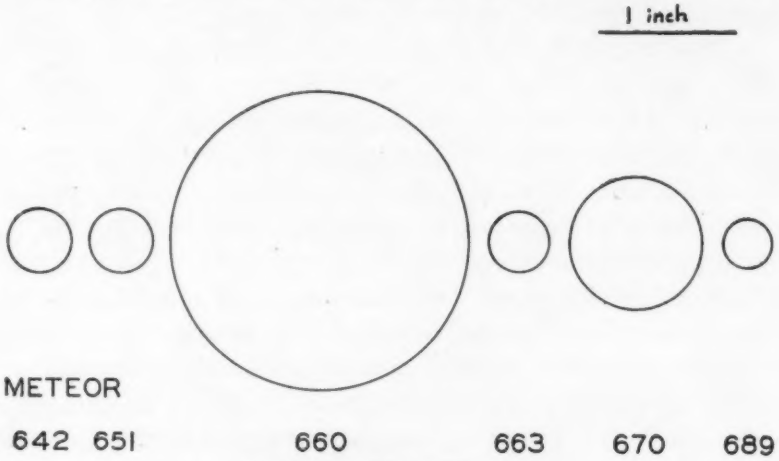

FIG. 4. Radii of meteoroids.

sure of 41 mm Hg and a temperature of − 58° C were assumed at $H = 20$ km.[40] Above this height, F. J. W. Whipple's [44] measures of the temperature up to 55 km were the fundamental basis of the scale, with allowance for Duckert's [45] somewhat higher values. These determinations of atmospheric temperature depend upon the velocity of propagation of sound waves from gun fire, and can be relied upon to about $H = 50$ km. Above this height the temperature continues to rise to about 60 km, according to F. J. W. Whipple, but a reversal probably occurs above 60 km. Gun-fire is rarely

[44] *Quart. J. Roy. Met. Soc.*, **57**, 331, 1931; **58**, 471, 1932; and **60**, 80, 1934.
[45] Gerlands *Beitr. z. Geophys. Suppl.*, **1**, 280, 1931.

reflected from, and detonating meteors are rarely heard at, higher levels.[46] In the theory developed by Taylor [47] and Pekeris,[9] a considerable temperature decrease above some level near 60 km is required to account for the two free periods of atmospheric oscillations. One is the observed period of 12^h and the other is $10^h.5$, corresponding to the observed velocities of sound waves from the Great Siberian Meteor and Krakatoa explosions.[48] Martyn and Pulley [49] find that when Gowan's [50] conclusions concerning ozone formation are corrected by Dobson's [51] revised estimates of the height, a temperature maximum near $H = 60$ km is suggested.

It is clear, then, that the atmospheric temperature distribution to above 60 km is fairly well defined by the observations mentioned above. The remaining problems of the amount of the temperature drop above this level and the subsequent rise at the auroral regions is less clearly defined. Much depends upon the origin of the noctilucent clouds. Humphreys [52] has suggested that these clouds are composed of ice crystals which he has shown will be formed over ice at a temperature of 160° K. Jesse [53] and Størmer [54] have shown that at different times (June or July) over a number of years the noctilucent clouds are found at a nearly constant height of 82 km. The complete range found by Størmer was from 75 to 88 km with a high concentration near 82 km. His observations of velocities averaging about 50 m/sec is convincing evidence for effective mixing of atmospheric constituents at these levels, and, consequently, for a homogeneous atmosphere to the heights considered in the present paper. As to the origin of the clouds, Størmer, with Vestine,[55] favors the idea that they

[46] F. J. W. Whipple, Gerlands *Beitr. Geophys.*, **24**, 72, 1929.
[47] *Proc. Roy. Soc.* A, **126**, 169, 728, 1929.
[48] F. J. W. Whipple, *Quart. J. Roy. Met. Soc.*, **56**, 287, 1930; and **60**, 505, 1934.
[49] *Proc. Roy. Soc.* A, **154**, 455, 1936.
[50] *Proc. Roy. Soc.* A, **128**, 531, 1930.
[51] *Proc. Roy. Soc.* A, **129**, 411, 1930.
[52] *Mon. Weather Rev.*, Washington, **61**, 228, 1933.
[53] *A. N.*, **140**, 161, 1896.
[54] *Univ. Obs. Oslo, Pub. No. 6*, 1933; and *Astr. Norv.*, **1**, 87, 1935.
[55] *J. R. A. S. Can.*, **28**, 249, 303, 1934.

are composed of cosmic dust rather than of ice crystals. Since the cosmic dust hypothesis gives no inkling as to the temperature at this level, a value of 180° K is adopted in order to approximate more closely the observed density at 100 km. It is possible that Humphreys' explanation may still apply at this temperature, that there are appreciable seasonal variations, or that the temperature is higher at less northerly latitudes where the noctilucent clouds are rarely observed. A minimum temperature of nearly 300° K at 82 km would be required to provide the density calculated at $H = 115$ km for the hyperbolic solution of Meteor 663. The adoption of the elliptical solution and corresponding density thus appears to be justified.

The assumed temperature distribution above the minimum at $H = 82$ km affects the calculated density at $H = 100$ km only slightly. Vegard's [56] calculations of temperatures in the auroral regions, from studies of nitrogen band spectra, have been corrected to 347° or higher by Rosseland and Steensholt.[57] A uniform increase of temperature from 180° at 82 km to 347° at 110 km is therefore adopted. The alternate lower-temperature curve in Fig. 3 passes through Vegard's measures uncorrected; the curve would become nearly vertical at greater heights. Öpik,[58] from statistical studies of meteor heights, places the temperature at 373° K at 90 km, a much greater value.

The above temperature distribution has been assumed in a fashion similar to that of Martyn and Pulley.[49] Their published temperature curve, unfortunately, does not agree with the limited number of temperatures that they mention, nor is it possible to reconstruct the distribution accurately from any data that they give. From radio methods they obtain a pressure of 0.001 mm (Hg) at $H = 105$ km. This value corresponds to $\log \rho = -8.8$, if $T = 300°$ as they assumed. In the present integration $\log \rho = -9.1$ at 105 km, representing a difference of 0.3 in the logarithm.

[56] *Geophys. Pub., Oslo, No. 9*, 49, 1932; and *Nature*, **138**, 930, 1936.
[57] *Univ. Obs., Oslo, Pub. No. 7*, 1933.
[58] *H. A.*, **105**, 549, 1937.

The adopted density distribution up to about 70 km agrees well with that obtained by Lindemann and Dobson [29] in their remarkable work on this subject. Above this height their calculations lead to densities that are appreciably greater than those adopted here. Martyn and Pulley, applying a suggestion by Hercus, have shown that a correction to the theory used by Lindemann and Dobson would, from their data, yield a density of 2.2×10^{-9} at $H = 105$ km. Actually, however, Lindemann and Dobson obtained greater values of the density at this and at higher altitudes than were assumed by Martyn and Pulley. Presumably, errors in even the excellent observations by Denning may have been the source of some of the remaining discrepancy. Öpik [58] has pointed out that random errors of observation tend to produce too large values for arithmetic mean heights.

From studies of the scattered light in the upper atmosphere, Link [59] has determined values for the atmospheric density that are in fair agreement with those adopted here, up to about $H = 50$ km. At $H = 100$ km, however, his calculated densities are nearly one hundred times greater. The method could hardly be expected to give precise results for the very rarefied upper atmosphere.

It should be noted that the density distribution adopted in the present paper is independent of my observations except at the greatest heights. The agreement between the observed crosses and the solid curve in Fig. 2 is thus a check upon the data and the assumptions by which both are calculated. The assumption of a constant molecular weight to the heights involved seems justified in view of the small number of electrons needed for radio wave reflections in the E-layer (see Martyn and Pulley),[49] the character of the auroral and night-sky spectra (see Vegard),[56] and the negligible quantity of ozone present in the entire atmosphere (see Dobson).[51] A factor of probably greater importance is the seasonal change in temperature and therefore in density of the atmosphere. We note that the densities calculated from the meteor data are syste-

[59] *Comptes Rendues*, 200, 78, 1935.

matically placed about the adopted density curve with respect to the season. The densities determined during the winter months are relatively smaller than those determined during the summer months. A least-squares solution was made for the amount of the effect, the uncertain determination (but the only one in summer) for Meteor 689 at $H = 91.8$ km being neglected. The minimum temperature was assumed to occur on January 20 and the variation to follow a sine law. The calculated amplitude in $\log \rho_0$ is 0.50 ± 0.12 (p.e.) with a systematic correction of -0.17 to the observed values of $\log \rho_0$. The deviations of the adopted density curve from the corrected observations are given in Table VII as $\Delta \log \rho_0$. The deviation for Meteor 689, not used in the determination of the seasonal and systematic corrections, is reduced from -0.62 (see Fig. 2) to -0.22; the other deviations are, in all cases, numerically smaller than before, the numerical average of all being ± 0.07.

Öpik [58] has found a seasonal amplitude of 3.7 ± 0.7 km in the observed heights of meteors near the 86 km level. The computed amplitude of 0.5 in $\log \rho$ corresponds to 5.7 ± 1.4 km at this height. In both cases the height or density follows the variations of lower-atmosphere temperatures in a direct manner, as though the whole upper atmosphere were raised with increasing temperature. Such seasonal variations of temperature are marked in the stratosphere [60] and are definitely present in the gun-fire determinations of temperature by F. J. W. Whipple.[60] The observed changes are in the correct sense and of the right order of magnitude to account for the observations from meteor data.

The systematic deviation of -0.2 in the observed values of $\log \rho$ may represent a lower limit to the true systematic error because the adopted density distribution may more nearly be a summer distribution than an average one. The zero-point data were observed in November but the adopted temperatures above this level are near F. J. W. Whipple's summer determinations. If, on the other hand, Duckert's

[60] *Quart. J. Roy. Met. Soc.*, **61**, 285, 1935.

higher values of the temperatures are more nearly correct, the adopted density curve may be close to the average. The systematic deviation of − 0.2 seems, however, satisfactorily small when one considers the large number of uncertain factors, theoretical and numerical, that enter into the calculation of both the adopted and meteoric density distributions in the atmosphere. Night and day effects may also enter. In addition, processes of melting and spraying in the disintegration of the meteor have been neglected. Their importance is difficult to calculate and may be considerable, as Öpik [28c] has shown.

A remaining point of interest concerns the determinations of density from the maximum-light data. Only the velocity, magnitude, and height at the time of maximum brightness were used in these calculations. A slight systematic correction was essentially applied through the determination of $\bar{\lambda}/\gamma$, but no smoothing effect could enter in this fashion. The deviations of the adopted density curve from log ρ_m, after the seasonal correction is applied, are given in the last row of Table VII. The numerical average is ± 0.16 and the systematic deviation − 0.06, in addition to the − 0.17 correction already applied. It appears that these determinations of density are nearly one-half as reliable as those obtained from the direct measurements of deceleration.

An important point is that when the height of maximum brightness and the corresponding magnitude of a meteor are known, the velocity can be calculated by means of Equation (26) with an accuracy of $\rho_m^{3/10}$. In the outstanding case of Meteor 651 the calculated velocity would have been in error by 35 per cent, in the average case by only 12 per cent. Thus two-camera or visual programs for the study of meteors can be made to provide excellent results concerning meteoric velocities without the use of any device for the direct determination of velocity and deceleration.

Summary

The results obtained from a study of the trails of the first six meteors photographed in a two-camera meteor program

recently instituted at the northern stations of the Harvard Observatory are presented. One camera, equipped with a rotating shutter, provided data for the accurate determination of velocities. The velocity measured for a Perseid-shower meteor agrees well with the known velocity of the associated comet. A Geminid is found to have been moving in an orbit with a period of 1.8 years. Of four sporadic meteors studied, three are found to have been moving in short-period orbits with very small inclinations to the plane of the ecliptic. These orbits are very similar to those of the asteroids with perihelion distances less than one astronomical unit. An asteroidal type of meteoroid is suggested. The fourth sporadic meteoroid was moving with retrograde motion in a nearly parabolic or possibly hyperbolic orbit of a cometary nature.

All of the meteoroids are found to have suffered small but measurable decelerations in passing through the upper atmosphere. The measures of deceleration, coupled with measures of velocity and luminosity, are sufficient for a rough determination of the fraction of the relative kinetic energy of the air molecules available for heating the nucleus. So much of the relative energy is expended in vaporizing the surface of a meteoroid that the core suffers only a small deceleration until the meteoroid is materially reduced in size. The theories by Öpik and Hoppe are the fundamental basis of these calculations and for the determinations of atmospheric densities at heights from fifty-eight to ninety-nine kilometers above the earth's surface. A density-distribution curve in the atmosphere is derived from other physical observations; the densities determined from the meteor data are compared with the values from this curve. A seasonal effect with a total amplitude of 0.5 in log density is suggested by the observations, the densities at a given height being greater in summer than in winter. A systematic deviation of -0.2 in $\log \rho$ exists between the two sets of density determinations, the meteoric data giving a greater density at a given level.

Formulæ are derived for the determination of meteor velocities from less complete observational material. It is

shown that a fairly reliable velocity can be obtained for an individual meteor from a knowledge of the radiant and the height and magnitude at the point of maximum light.

NOTATION

x	Coordinate on photographic plate parallel to meteor trail.
y	Coordinate on photographic plate perpendicular to meteor trail.
s	Distance in space along meteor trail.
A, B, and C	Coefficients of time terms in equations for meteor's motion.
t	Time (in Mean Time seconds).
d	Coefficient of t in exponential term of deceleration.
V	Velocity of meteor with respect to the observer.
V_∞	Relative velocity V, corrected for atmospheric deceleration.
V_G	Velocity V_∞ corrected for earth's attraction and for aberration.
V_H	Velocity with respect to the sun after earth effects are removed.
V'	dV/dt.
H	Height above sea level.
Subscript 0	Values at middle of observed interval.
Subscript m	Values at maximum light of meteor.
Subscript ∞	Values when atmospheric effects are removed.
Q	Angle between great-circle motions as seen from the two stations.
Z_R	Zenith distance of the radiant.
λ	Elongation of corrected radiant from apex of earth's motion.
q, a, e, i, ω, Ω	Orbital elements with customary significance.
n	Number of stars per sq. degree to limiting magnitude.
ρ	Density of air.
ρ'	Density of meteoroid.
V_a	Velocity of air after encounter with meteoroid.
m	Mass of meteoroid (or remaining nucleus).
m_a	Mass of air encountered.
A'	$(9\pi/16\rho'^2)$ for a sphere.
ζ	Heat of evaporation (assumed value: 6×10^{10} ergs gm^{-1}).
dB	Increment of kinetic energy.
dW	Increment of heat energy.
$\bar{\eta}^2$, $\bar{\lambda}$, γ	Numerical constants defined by Equation (6).
a, b	Physical constants of Atmospheric-Density Equation (13).
τ	Luminous efficiency factor.
τ_0	Constant of Luminous-Efficiency-Factor Equation (18).
I	"Visual" intensity of meteor.
T	Absolute temperature.
M_{pg}	Absolute photographic magnitude (app. mag. at 100 km).
M_v	Absolute visual magnitude.
r_∞	Radius of the meteoroid in space (assumed sphere with $\rho' = 3.4$)

THE BEARING OF NECTARIES ON THE PHYLOGENY OF FLOWERING PLANTS[1]

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(Communicated by Edward W. Berry)

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ABSTRACT

Nectaries appear to have arisen independently in different lines of development and then to have undergone modifications characteristic of various groups. Some exceptional conditions have been evolved, but these have been comparatively few.

The characteristic nectaries of the monocotyledons are septal glands. They run through the nectariferous *Palmae* and the orders *Liliales*, *Agavales*, *Dioscoreales*, *Amaryllidales*, *Iridales*, *Bromeliales*, and *Zingiberales*.

From the *Theales* there are two important lines. (1) The development of a disc around the base of the ovary. This type of nectary runs from the *Theales* through the *Ericales*, *Polemoniales*, *Boraginales*, *Solanales*, *Personales*, and *Lamiales*. The position of the nectary seems to be connected with the scarcity of inferior ovaries in these orders. Exceptions occur in the *Vacciniaceae* and in some of the genera of *Gesneriaceae*. In these cases, it seems probable that the nectaries became detached from the ovaries before the development of epigynous flowers. (2) The *Theales* also appear to have given rise to a condition in which the nectary is a ring beneath the stamens. This, with few exceptions, is found in the *Caryophyllales*, *Polygonales*, and nectariferous *Chenopodiales*.

In the *Tiliales*, the characteristic nectary is a cushion of multicellular hairs which in the *Tiliaceae* is located in various regions, but the location of which is stereotyped in the sepals of the *Malvales*, *Bombacaceae*, and *Sterculiaceae*. The nectar is made available through slits between the petals. The position of the nectaries is probably connected with the lack of gamopetalous flowers in this order.

The significance of the cup-shaped torus of the *Rosales* is that it is a nectary between the ovary and stamens. In one line, the nectary has grown up around and over the ovary in such a way as to lead through perigyny to epigyny, culminating in the *Compositae*. In another line the more basal part of the cup becomes united to the ovary leading to such orders as *Lythrales*, *Thymelaeales*, and *Myrtales*.

In *Flacourtiaceae* of the *Bixales* the nectary is either a disc attached to the torus or a modification of such a disc. From them the nectaries indicate two lines of development to inferior ovaries. (1) An herbaceous line through the *Passiflorales* to the *Cucurbitales* and (2) a woody line through the *Samydaceae* to the *Datiscaceae* and probably through them to the *Begoniaceae*.

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The type of nectaries found in the *Violales* and *Salicales* indicates that these two orders are related to the *Bixales* or *Rhoeadales*.

In the *Capparidaceae* the primitive type of ovary would appear to be similar to that in the *Flacourtiaceae*. Evolution in the *Rhoeadales* has probably proceeded from such woody forms with disc-shaped nectaries to herbaceous forms with nectaries composed of separate glands or single glands attached to one side of the torus.

The type of nectaries found in the *Laurales*, *Berberidales*, and *Ranunculales* indicates that these three orders are related. The *Laurales* represent an epigynous development; the other two, an hypogynous one in which the *Berberidales* are more woody and the *Ranunculales* more herbaceous. Nectaries in the *Geraniales* afford some reason for believing that this order may have been related to the complex of orders just discussed.

NECTARIES have aroused very little interest among botanists and their study has been very largely neglected. This seems very strange when we consider the importance of nectaries to plants themselves. A very high percentage of plants which are pollinated by insects are visited for their nectar. Where nectaries have been studied, this has not been done in reference to phylogeny except in special cases. Müller (1) made a great many observations on the occurrence of nectaries, but his chief interest was in mechanisms favoring cross-fertilization. His observations are trustworthy and very useful, but his discussions of nectaries are often very vague. In studying nectaries it is almost essential to use living material. The conclusions here presented are, unless otherwise stated, the results of my own observations on living plants of more than 3000 species. They have, of course, been checked with the literature on the subject.

The lack of interest in nectaries on the part of systematic botanists is probably the natural consequence of the fact that systematists have had to make most of their studies on dried plants. With such material, it is naturally impossible to observe the secretion of nectar.

An examination of nectaries shows that although they have been very important in the evolutionary development of flowering plants, they are probably not characteristic of the simplest of them. This would certainly be true according to the view of those botanists who regard the Amentiferae as primitive dicotyledons. It would, also, be true according to

the view which holds that the *Magnolias* are the most primitive flowering plants. Nectar has not been previously reported in any of the *Magnoliaceae* although insects may be attracted by stigmatic secretions.

Among them, I have found nectaries only in *Magnolia coco* (Fig. 1) and in *Talauma*, both of which approach *Anonaceae*. In *Magnolia coco* the perianth is really cyclic, there being three whorls of three members. The outer whorl is distinctly differentiated as a calyx while the members of the next whorl alternate with those of the calyx, and those of the innermost whorl with those of the second whorl. The two inner whorls are fleshy and really petals. There is very slight distinction between this flower and that of the *Anonaceae*. Very striking is the fact that the innermost petals secrete nectar as do those of many of the *Anonaceae*. In *Talauma*, which is very much like *Magnolia coco* except that the carpels are fused, the inner petals secrete nectar. It would seem that the line of evolution which produced *Magnolia coco* and *Talauma* continued and resulted in the *Anonales*; but there is, perhaps, no reason to believe that it gave rise to other groups.

Septal Glands

As an example of the constancy of nectaries, the septal glands of monocotyledons may be mentioned. These are much more widely distributed than has been generally realized. Septal glands occur in the septa between two carpels and represent places where the adjacent walls of the two carpels have not fused. They discharge nectar to the outside by means of small openings (Figs. 2, 3). They are such complicated structures that they would seem to indicate a relationship between all plants having them. Septal glands are characteristic of those *Palmae* (Figs. 4, 5) which are nectariferous and of the *Liliales* (Figs. 2, 3), *Agavales* (Fig. 6), *Dioscoreales*, *Amaryllidales* (Figs. 7, 8), *Iridales* (Fig. 9), *Bromeliales* (Figs. 10–12), and *Zingiberales* (Fig. 13).

Septal glands are not simply scattered through these orders, but predominate, and in many families there are no

other types of glands. I know of no exceptions in the *Dioscoreaceae, Agavaceae, Taccaceae, Bromeliaceae, Musaceae, Cannaceae, Pontederiaceae,* and *Marantaceae.* There are, however, a few exceptions which are worth considering so as to give a general picture of the nectaries in these orders.

In the family *Zingiberaceae* of the *Zingiberales* the characteristic nectaries are two long glands which project from positions where septal glands, if present, would be expected to discharge. In the simplest of the *Zingiberaceae,* the nectaries are typical septal glands. In these the nectariferous tissue is at the bottom of the gland cavity and is dome-shaped (Fig. 13). In more advanced genera, the dome-shaped tissue seems to have grown out into the elongated glands which are characteristic of the family. In still more specialized types, the two glands fuse to form a ring (Fig. 14). Thus the development of the characteristic glands of the *Zingiberaceae* can be traced back to septal glands.

In the *Amaryllidaceae* the nectaries are very uniformly septal glands. An exception has been reported in the snowdrop, *Galanthus nivalis L.,* where according to Knuth (2), the nectar is secreted by grooves in the inner perianth leaves.

In the *Iridaceae* septal glands are of wide occurrence. In *Iris* and some other genera, nectar is secreted by the lower part of the corolla tube. This is perhaps a modification of septal glands. In *Belamcanda chinensis* nectar is secreted by grooves in the outer sections of the perianth (Fig. 15).

In the *Tulipeae* of the *Liliaceae,* the nectar glands, in many cases, occur in grooves running along the perianth leaves (Fig. 16). This is a very conspicuous tribe and contains such plants as *Tulipa, Lilium, Hyacinthus,* and numerous others. The occurrence of nectaries in the perianth rather than the septa would indicate that this tribe has played little part in the further development of nectariferous monocotyledons.

In the male flowers of *Smilax bracteata* Presl., the bases of the stamens are nectariferous; while in the female flowers the staminodes are nectar glands. These types of nectar glands

may afford some justification for the removal of the *Smilacaceae* from the *Liliaceae* and placing them in a separate family by Hutchinson (3).

The openings of septal glands may be near the base of the ovary, but are often at the top of the ovary or even in the style. Where the glands open at the top of the ovary, it would seem that they would not interfere with the fusion of the ovary and torus. This may be connected with the fact that the higher monocotyledons frequently have inferior ovaries. In the next line of development to be discussed, the position of the nectaries appears to render the development of inferior ovaries less likely.

The probable origin of septal glands is of considerable interest. The condition of the nectaries in the *Alismaceae*, usually considered the most primitive of the monocotyledons, may be illustrated by *Sagittaria sagittaefolia* L. The female flowers have glandular tissue composed of clear, thin-walled, bulging cells which are found between petals and staminodes and between and around the staminodes and lower carpels (Figs. 17, 18). Another very interesting condition is found in the flowers of *Alisma plantago*. Here the carpels are arranged very close together in a circle. The bottom of the slit between two adjacent carpels is lined with large, rather-clear, bulging cells similar to those in *Sagittaria*. As in *Sagittaria*, they secrete nectar, a drop of which exudes from the opening between the bases of two adjacent carpels. The fusion of these carpels would bring about a condition found in the more primitive or bisexual flowers of the *Palmales*. Here the carpels are very imperfectly fused being united only at the base and summit of the ovary, leaving conspicuous slits between the carpels (Fig. 19). The bottoms of these slits are lined with glandular cells (Fig. 20) similar to those found in the *Alismaceae*. Apparently a union of the carpels has resulted in the typical septal glands in more advanced nectariferous palms. The fact that the nectaries in simple palms appear to be a developmental stage of septal glands would indicate that the separation between the *Palmales* and

Liliales must have occurred early and that the palms can not be derived from typical *Liliales*. The palms are much older geologically.

While the above discussion may indicate the method of origin of septal glands, this should not be taken to indicate that the palms have been derived from the *Alismaceae*. Hutchinson (3) appears to have made a fundamental division among the monocotyledoneae when he separates *Calyciferae* (calyx-bearers) from *Corolliferae* (corolla-bearers). In the first the calyx is very decidedly different from the corolla. In the *Corolliferae*, calyx and corolla are much alike. The *Alismaceae* belong in the former group, and the *Palmae* in the latter. As the palms are so very ancient geologically, and are woody in structure; it is possible that they represent an ancient line from which the herbaceous forms are descended.

The fact that septal glands must have had their origin before the fusion of the carpels would seem to indicate that flowers with septal glands can hardly have originated from flowers with fused carpels and no nectaries, and so that Hutchinson's suggestion that the *Bromeliales* and *Zingiberales* are derived from the Commelinales, where there is no nectar, is probably not correct (3).

Theales and Their Descendants

In examining a considerable number of the *Dilleniaceae*, which are generally conceded to be very close to the *Magnoliaceae*, I have not found nectar; but would not be surprised if it were present in some genera. According to Hutchinson's system, the *Guttiferales* and *Theales* are derived from the *Dilleniales*, a position which is in keeping with widely accepted opinion.

In many of the *Guttiferales*, there is no nectar. In some there are nectar glands which seem to be modified groups of stamens. This might indicate that in the case of *Guttiferales*, the production of nectar originated within the order; but it is, of course, possible that it had its origin earlier, as there is considerable similarity between *Guttiferales* and *Theales* in this respect.

The *Theales* are more interesting as they appear to have given rise to a very extensive line of evolution. In some *Theales*, the more basal parts of the stamens secrete nectar (Fig. 21). There appears to be little modification of structure other than that the contents of the nectariferous region are slightly dense. From this there are two tendencies which have been very important. One is the adherence of nectariferous tissue to the basal portion of the ovary as seen in the pistillate flower of *Euyra japonica* (Fig. 22). Here the basal part of the ovary secretes nectar very abundantly, although externally no disc is evident. The condition here is very similar indeed to that of the *Clethraceae* (Figs. 23, 24). *Clethra* is reported to be without a disc, but the lower part of the ovary in *Clethra lancifolia* Turcz. is glandular and secretes nectar abundantly (Fig. 24). The general characters of the *Theaceae* and *Clethraceae* are very similar and there is no great difference in general floral characters. The secretory region which in *Clethra* is not externally evident except from its secretions, becomes a disc in other *Ericaceae*. In some, the disc is barely visable, while in others it becomes conspicuous. In still others it is a distinct structure which is practically separate from the ovary itself (Fig. 25). It is usually a conspicuous disc around the base of the ovary, and attached to the ovary (Fig. 26). From some such form as the generalized *Ericales*, it is an easy step to the *Polemoniales* (Fig. 28), *Boraginales* (Fig. 27), *Solanales*, *Personales*, and *Lamiales* (Fig. 29), in all of which a nectary disc attached to the base of the ovary is characteristic. This group of orders is conspicuous as including a very large number of highly developed and successful families with united corollas and superior ovaries. Probably the great predominance of superior ovaries in this group is connected with the occurrence of a nectary disc around the base of the ovary as a fusion of torus or calyx to the ovary would cover up the nectary.

Exceptions to the superior position of the ovary are found in the family *Vacciniaceae* of the *Ericales* and in some genera of the *Gesneriaceae* of the order *Personales*. As has been

mentioned, in some of the *Ericaceae* the disc which is typically attached to the base of the ovary may become detached and joined only to the torus. Such a condition as this may have made it possible for the torus to grow up around the ovary without enclosing the nectary. Similar conditions prevail in the *Gesneriaceae*.

We do not necessarily have to assume that the *Clethraceae* and the *Ericaceae* are the actual transitional forms. In fact, the peculiar stamens of this group argue against this assumption. There are other primitive hypogynous forms with nectaries similar to those of *Clethra*.

Another tendency shown by *Theales* is the aggregation of nectariferous tissue as a ring below the stamens on the inner side. This condition can be found in the *Caryophyllales* (Figs. 30, 31), *Polygonales* (Fig. 32), *Nyctaginaceae*, and the nectariferous *Chenopodiales* (Fig. 33) where the nectary or nectariferous tissue is a definite ring below the stamens.

The position of the nectaries in the *Caryophyllales* is very different from that in the *Polemoniales*, etc. This suggests at once that the latter could not be derived from the former as in the usual schemes of classification, while the derivation of both from the *Theales* is very logical.

While the proposed scheme as a whole seems somewhat revolutionary; if the individual changes are examined, they are not so startling. Thus Hutchinson's (3) derivation of the *Ericales* from the *Theales* is in agreement with the scheme here presented. Pool (4) in interpreting Bessey's system derives the *Ericales* from the *Caryophyllales*. The new scheme simply reverses this order of derivation.

Sinnot and Bailey (5) and many others have shown clearly that the tendency of evolution in the dicotyledons, at least usually, has been from woody to herbaceous forms. This tendency, however, has not been made use of in systems of classifications. Against the prevailing schemes of deriving the herbaceous order *Caryophyllales* from the *Ranunculaceae* and from the *Caryophyllales*, the *Boraginales*, *Solanales*, *Personales*, and *Lamiales*, it should be remembered that the

latter contain very many large woody trees. A botanist from the tropics would certainly consider *Boraginaceae* and *Verbenaceae* as woody families. The *Verbenaceae* contain such well-known woods as teak and many others even harder. To derive such woody families from the *Ranunculaceae* through the herbaceous *Caryophyllales* would certainly be contrary to the general trend from woody to herbaceous. Bailey and Sinnot point out that according to Engler's and Prantl's account the more primitive of the *Polemoniales* and *Boraginales* are woody. From the standpoint of woody structure it is certainly logical to go from the woody *Magnoliales* through the woody *Dilleniales*, *Theales*, and *Ericales* to woody *Polemoniales* and from these to woody *Boraginales*, *Solanales*, and *Personales*, and then to derive the *Lamiales* and the more herbaceous members of the various orders from the woody ones. Another very serious objection to deriving the *Polemoniales*, *Boraginales*, etc. from the *Caryophyllales* is that in the latter placentation tends to be central, or rather basal, and it is hard to see how orders with axile placentation can be derived from them.

As to the relation of the *Caryophyllales* to the *Ranunculaceae*, it may be said that the flowers in the two groups are so very dissimilar that any near relationship is certainly very doubtful.

While the character of the nectary is very constant in the groups which we have been considering, there is an interesting exception in the *Verbenaceae*. In *Avicennia* the ovary has the usual basal swelling which is generally nectariferous in *Verbenaceae* (Fig. 34). In *Avicennia* it is not secretory, but the corolla exudes abundant nectar. This comes from glandular hairs indicating that here glandular hairs on the petals have taken the place of the nectary at the base of the ovary.

Tiliales

Before leaving the descendants of the *Dilleniales*, we may mention the *Tiliales* which are often derived from that group. In this order, nectary glands are characteristic, multicellular,

glandular hairs which are usually packed close together to form cushion-like growths (Fig. 35). In the *Tiliaceae*, these nectaries are found in various places including the sepals, petals, and androgynophore. In the *Malvaceae*, *Bombacaceae*, and *Sterculiaceae* they have become localized and are found in the sepals (Fig. 36). In the *Malvaceae* and *Bombacaceae*, and particularly in the *Malvaceae* the structure of the flower has become stereotyped. The honey is made available by means of openings between overlapping bases of the petals (Fig. 37). The lack of gamopetalous corollas in these families is probably connected with the occurrence of nectaries in the sepals and the necessity for slits between the petals to make the nectar available. In the *Sterculiaceae* the form of the flower, instead of being stereotyped as in the *Malvaceae*, is very varied and there are even horizontal "tubular" flowers as in *Helicteres* (Fig. 38). Still, there is no union of petals and slits between the petals serve for the entry of nectar to the base of the "tube." The lack of strength which would be given by a gamopetalous corolla is compensated in *Helicteres* by flaps on either side of each petals (Fig. 39). The flaps on one side grasp the next petal while the flaps from the other side fold around the style. In this flower the advantage of a horizontal tubular corolla is certainly produced in a very complicated manner.

In the *Malvales* the positions of the nectary glands would seem to be clearly connected with the lack of gamopetally in this order.

Rosales and Their Descendants

In the Rosales, the significance of the characteristic cup-shaped torus is that it is a nectary located between the carpels and the stamens (Figs. 40, 41). This type of nectary is found in those *Rosaceae* which have completely superior ovaries. It seems, however, to have given rise to a great many plants that have partly-inferior or inferior ovaries. This has taken place in two ways.

Often the nectariferous part of the cup has become enlarged and partly or wholly surrounds the ovary, rendering it

partly or wholly inferior. This would seem to be the origin of the *Saxifragales* (Fig. 42), *Celastrales* (Figs. 43, 44), *Rhamnales* (Fig. 45), and *Olacales* (Fig. 46), where, usually, the nectary partially surrounds the ovary. Much the same condition is found in the *Sapindales* (Fig. 47), while the *Rutales* (Figs. 48, 49) and *Meliales* (Fig. 50) would appear to have had a similar origin.

In many of the *Euphorbiaceae*, the nectary is a disc (Figs. 51, 52, 53) in very much the same position as it is in the *Sapindales*. This derivation of the *Euphorbiales* seems probable.

There is good evidence for deriving the *Juglandales* from about the same level as the *Sapindales*. In this case it is only necessary to assume that there was a degeneration of the nectary along with the development of adaption for wind-pollination, a process which seems to be taking place in some of the *Aceraceae*.

A tendency for the nectary to grow over the ovary as seen in the *Celastrales* appears to have developed further and given rise to the *Umbellales* where the nectaries are above the ovaries and the latter is inferior (Figs. 54, 55, 56, 57). Still, further development of this line has led to the *Rubiales* (Fig. 58), *Campanulales* (Fig. 59), and *Asterales* (Figs. 60, 61).

The uniformity with which the nectary of the *Rubiaceae* is a cushion above the ovary is indeed striking. A slight variation occurs in the case of *Uncaria philippinensis* where the nectary is attached not only to the top of the ovary but also to the basal portion of the corolla tube (Fig. 62).

In the *Caprifoliaceae*, there are some interesting exceptions. The nectary consists of a single lobe on one side of the style in *Weigelia florida*. In *Lonicera* and *Kolkwitzia amabilis* nectar is secreted by glandular hairs in the corolla tube. These form an elongated patch near the base of the tube on the lower side of the flower. In some species of *Lonicera* and in *Kolkwitzia amabilis*, there is a slight indentation where these hairs occur and particularly in the lower part of the glandular region. This would seem to be the same type of tendency which has produced the *Valerianaceae*. This latter family should

probably be placed in the same order as the *Caprifoliaceae* instead of the two being in separate orders as has been suggested.

In *Scabiosa* of the *Dipsacaceae* (Order *Asterales*) there is a nectary in the usual position for this group of orders and also an extension of the nectary which runs up one side of the corolla tube. It is to be noted that in the densely packed flowerheads of *Scabiosa* the space around the base of the style is very small and the extension of the nectary increases the secreting surface considerably.

In the *Compositae* the nectary is very uniformly a tubular structure at the top of the ovary and around the base of the style (Figs. 60, 61).

Another method by which the ovaries of the descendants of *Rosales* have become inferior has been for the basal portion of the torus cup to become fused to the ovary. Perhaps this has been accompanied by some tendency for the secretory region of the cup to become localized near the rim.

A good example of a long torus cup in which the lower part is not nectariferous is afforded by *Elaeagnus* of the *Elaeagnaceae*. Here the cup is really a long tube. The lower part of the tube surrounds the ovary very closely. A constriction occurs in the tube above the ovary, and it is in this constriction or just above it that the secretion of nectar takes place. Here there has been no fusion of ovary and torus tube. However, such a fusion would not interfere with the production of nectar. In other cases, there has been an actual fusion of torus cup and ovary with the production of partly or completely inferior ovaries. The development of a tubular torus has resulted in the *Lythrales* and *Thymelaeales* (Fig. 63). A fusion of the basal part of the torus cup to the ovary would seem to be responsible for the *Myrtales* (Fig. 64) including the families *Sonneratiaceae*, *Punicaceae*, *Rhizophoraceae* (Fig. 65), *Combretaceae* (Fig. 66), *Lecythicaceae*, *Myrtaceae* (Fig. 67), *Melastomaceae*, and *Onagraceae* (Fig. 68).

The use of a nectary in settling the systematic position of a family is well illustrated in the case of *Nyctaginaceae*. These

are placed by Hutchinson in the *Thymelaeaceae*. The nectaries in *Nyctaginaceae* are very typical of those of the *Caryophyllales* and very different from anything found in the *Thymelaeaceae* or any of its alliances. This is so true of this character that taken alone it seems to place the *Nyctaginaceae* in the *Caryophyllales*.

Origin of Disc in the Rosales

It is much easier to trace the development of the disc of the *Rosales* in their descendants than it is to determine how the disc itself originated. Certainly, the *Magnoliales* are hypogynous rather than epigynous and the development of epigyny in the *Rosales* would appear to have been connected with the development of the nectary disc.

In the flowers of *Illicium*, family *Winteraceae* of the *Magnoliales*, there is a rounded torus around which the carpels are attached in a circular arrangement. The central torus has been reported by Delpino (6) as being covered by little papillae which are licked by an insect. From his description this region is evidently a nectary on the torus.

In some (*Rourea erecta*, *Cnestis palala*, Fig. 69) of the *Connaraceae* there is also a very similar arrangement of the nectary. In them a central part of the torus is surrounded by a whorl of carpels and the central portion is covered by rounded, projecting, nectariferous cells. In *Agelaea everettii* the nectary covers more of the torus and extends between the carpels and slightly around the outside of them. In *Connarus neurocalyx* the nectary is a ring around the torus at the base of the stamens and outside of them. In the *Connaraceae*, which might be expected to be a primitive group, there is certainly considerable variation in the area which is covered by the nectary. In this family, although there are five separate carpels, only one of these develops into a fruit.

In the *Leguminosae* there is only one carpel. The latter group has long been regarded as derived from the *Connaraceae*. In the *Connaraceae* the disappearance of the four carpels, which do not produce fruit, would certainly result in a condi-

tion similar to that of the *Leguminosae*. Also, there would be a lopsided nectary such as is characteristic of the *Leguminosae*. As the nectary in those *Connaraceae* studied has shown no evidence of being such a cup-shaped structure as in the *Rosaceae*, it seems improbable that the *Connaraceae* stand between the *Rosaceae* and the *Leguminosae*. It would appear, however, that the *Leguminosae* could be derived from some such form as *Illicium* through the *Connaraceae*. The same origin would not seem probable for those *Rosales* in which the carpels have a spiral arrangement. However, they may have had a somewhat similar origin.

The genus *Paeonia* may be interesting in this connection. Worsdell (7), after studying the petals and anatomy of the *Paeonia*, has concluded that its alliance is not with the *Ranunculaceae* but rather with the *Magnoliaceae*. In the former group petals are modified stamens while the petals of the *Paeonia* show no indication of such an origin. In most of the species of this genus nectar secreting structures have not been reported, but around the carpels there is a sort of disc and the stamens are inserted in depressions in this disc. In the tree paeony, *Paeonia Moutan*, this disc is very conspicuous. This species, according to Delpino (1), is regularly fertilized by Cetoniae which lick the fleshy disc around the carpels. Here, as in the *Connaraceae*, the nectary is a development of the torus and it is not impossible that the nectary disc here and in the *Rosales* may have had a similar origin. Before such a conclusion can be accepted, however, more information is necessary.

Bixales and Their Descendants

The *Bixaceae* and the *Flacourtiaceae* of the order *Bixales* represent another line of development in which the nectary is an outgrowth of the torus. In Bixa (Fig. 70) the stamens are located on the top of a disc just around the ovary. The interior of this disc, that is, the part around the ovary, may secrete nectar very abundantly. The most abundant secretion of nectar is thus between the ovary and the stamens, a

position which is very suggestive of that of the *Rosales*. The general arrangement of stamens on a disc is also suggestive of the condition in *Paeonia*. Whether these similarities suggest any close relationship is problematical. They perhaps suggest some sort of relationship, although this may be remote.

In the *Flacourtiaceae*, the nectary disc is very variable indeed. It may be a disc to which the stamens are attached as in *Scolopia luzonensis* (Fig. 71) or a disc around the stamens as in *Flacourtia rukam* (Fig. 72). The disc may be composed of lobes as in *Osmelia philippina* and *Flacourtia indica*. In some types, as in *Hydnocarpus alcalae*, the nectary consists of separate complicated apendages. Again in *Ahernia glandulosa* (Fig. 73) it is composed of separate glands interspersed among the stamens, the glands being generally back of the stamens. All of these forms would seem to be variations of a disc attached to the torus. The variable *Flacourtiaceae* appear to be more primitive than the *Bixaceae*. The latter, with its bilocular ovary, is probably an offshoot from the former.

Tamarix of the *Tamaricales* has a prominent circular nectary disc attached to the torus between the ovary and stamens. This would be in keeping with a derivation of the *Tamaricales* from the *Bixales*.

In *Passiflorales*, an order which is generally placed after the *Bixales*, the nectary is, really, a modified disc attached to the central axis and lining the torus cup.

In the *Cucurbitaceae*, which are usually regarded as related to the *Passiflorales*, the nectary is also an outgrowth of the torus.

In the male flower of *Citrullus vulgaris* there is a disc in the center of the flower enclosed within the calyx cup and surrounded by the stamens. In the female flower this disc is within the staminodes and enclosed by the torus cup which surrounds the base of the style.

In the male flower of *Cucumis melo*, the nectary is a raised disc in the center of the flower. It is below the anthers and within the attachements of the stamens. The female flower is very similar to that of *Cucumis vulgaris*. In the female

flower of *Cucumis sativus* the nectary is a ring around the base of the style and, in the male flower, is a three-lobed structure in the center of the flower. The three-lobed condition suggests that this structure is a combination of a rudimentary ovary and a nectary. This may be true in the male flower of some other *Cucurbitaceae*. In the female flower of *Momordica charantia* the nectary consists of oval-shaped glands which are attached to the torus and form a circle around the base of the stigma. In the female flowers, the nectary is at the base of the torus cup and is a cup-shaped lining of the torus. In the female flower of *Luffa cylindrica* the nectary is again a ring attached to the base of the torus cup and extending around the base of the style. In the male flower it is a cup lining the bottom of the torus cup. This cup is so arched over by the bases of the stamens as to be in a cavity. In the female flower of *Sechium edule* the nectary is a star-shaped disc around the base of the style, while in the male flowers the nectary lines a circle of little cavities in a disc. Altogether the evidence from the nectaries in the *Cucurbitaceae* is in agreement with the idea that this group is related to the *Passifloraceae* and that they may be derived from the Bixales.

The family *Samydaceae* of the *Bixales* may be regarded as the beginning of a line of development which has gone through the *Datiscaceae* to the *Begoniaceae*. Theoretically, *Casearia* affords a good starting point. Here, there is a circle of stamens alternating with a circle of glands which are interior to the stamens, the two forming a sort of corona. The lower part of the glands secrete nectar abundantly on the inner side and form a cup around the base of the ovary. In *Homalium* the ovary becomes more than half-inferior. In *Octomeles* of the *Datiscaceae* the ovary is completely inferior. The nectary has much the same relation to the stamens as in *Casearia*, in that the nectary is a lobed disc, the lobes alternating with the stamens or pistils as the case may be. The lobes of the disc do not have sterile prolongation as in *Casearia*.

The above discussion would appear to indicate that from the *Flacourtiaceae* there have been two lines of development

which have led to inferior ovaries. One, an herbaceous development through the *Passiflorales* to the *Cucurbitales* and, the other, a woody line of development through the *Samydaceae* to the *Datiscaceae*. The latter appear to be very close to the *Begoniaceae*. These two families agree not only in having parietal placentation and inferior ovaries, but also in having unisexual flowers. The *Begoniaceae* would appear to represent an herbaceous development from a woody ancestor. The family contains very succulent representatives but also shrubby ones.

It would seem logical to place the *Datiscaceae* and *Begoniaceae* in a separate order, the *Datiscales*.

The *Violales* are a group which has been variously placed. Often they have been put near the *Bixales* on account of their typical parietal placentation. The nectaries afford very strong support for this position. In some *Flacourtiaceae*, as in *Ahernia* (Fig. 73), the nectaries may be individual glands back of individual stamens. In the tree *Violaceae* with regular flowers there are genera with glandular structure appendaged to the stamens. This condition is intermediate between that in the *Flacourtiaceae* and in the genus *Viola* where spurs of the anterior anthers secrete nectar. This would seem to link the *Violaceae* with the *Flacourtiaceae*.

Rhoeadales

The order *Rhoeadales* has often been included with the groups just discussed to form the *Parietales*, as both groups have typical parietal placentation. The *Rhoeadales*, like the *Bixales*, have nectaries which are outgrowths of the torus. This may show some connection although the flowers of the two orders are different enough so that it is hardly reasonable to suppose that either was derived from the other.

In the *Rhoeadales*, the *Papaveraceae* are often placed first and after these, the *Capparidaceae*. This is perhaps a survival of the old idea that in the Angiosperms evolution has gone from herbaceous to woody rather than the more modern conception that in general the evolution of this group has been in the reverse direction.

The *Capparidaceae* contain small trees with altogether woody stems. The nectaries in the genus *Capparis* vary from a circular disc around the base of the stamens to a knob on one side of the torus. In *Capparis micracantha* it is a small rounded growth on one side of the flower. In *Capparis lobbiana* (Fig. 74) and in *Crataeva religiosa* (Fig. 75), the nectary is a disc around and beneath the stamens. These conditions are certainly very suggestive of the *Bixales*.

In many of the more herbaceous *Capparidaceae* (Fig. 76), the ovaries are on one side of the torus as is also true in the *Resedaceae*. The *Cruciferae* have such a fixed specialized type of flower that they can hardly be regarded as primitive. The nectary consists of glands attached to the torus. These may, perhaps, be derived from a ring-shaped disc. Dr. E. W. Berry regards the *Cruciferae* as chiefly post-glacial. Altogether it is logical to believe that evolution in the *Capparidales* has been from woody forms to herbaceous ones.

Salicales

It has been suggested that the *Salicales* are derivatives of the *Flacourtiaceae* as both have parietal placentation. The placentation of the two groups and of the *Rhoeadales* is certainly very similar. The glands of the *Salicaceae* are very much like those of some of the *Capparidaceae* and *Flacourtiaceae*, and afford strong evidence for placing the *Salicaceae* near one of the latter. The glands of the *Salicaceae* are certainly elongated specialized structures and unless this family is related to the *Rhoeadales* or *Bixales* no developmental history for them is evident.

Miss Fisher (8) has made a very thorough and interesting study of the morphology and anatomy of the flowers of the *Salicaceae* in which she shows that the nectary of *Salix* may have a vascular supply. She quotes this as evidence that the nectary is derived from a perianth, and assumes that the perianth was not originally nectariferous. However, her evidence is not contrary to such an alliance as here suggested.

In both the *Bixales* and *Rhoeadales*, where stamens are attached to nectariferous discs, veins to the stamens may go

through the discs. Also, it is quite common for nectary discs to have separate vascular supplies. These may, of course, be vestiges of former organs, such as stamens. In the *Passifloraceae* and *Cucurbitaceae*, the nectaries are abundantly supplied with veins. The glands of the *Resedaceae* also have vascular supplies.

Polanisia trachysperma, of the *Capparidaceae*, has a single nectar gland with a shape very much like that of many of the species of *Salix*. This gland has a pronounced vascular supply running high up into it.

A vascular supply into nectar glands is so characteristic of the *Rhoeadales* and *Bixales* and their descendants, that this may be considered as characteristic of many families of these two lines; and may afford some additional evidence of relationship between the two.

Nectaries Derived from Individual Stamens

In the *Laurales*, *Berberidales*, and *Ranunculales*, most of the insect-pollinated genera have nectary glands that are modified stamens. This together with other evidence indicates that all of this group may be related. Primitively these groups do not appear to have possessed petals and, where they do, there is strong evidence that these petals are modified honey leaves which in turn are modified stamens. In this group, the *Laurales* represent a perigynous development (Figs. 77, 78). If they are derived from *Magnoliales*, it must have been from a group in which petals had not been differentiated from the sepals. In some of the *Monimiaceae* and *Myristaceae* the torus cup serves as a nectary. In other *Monimiaceae*, and in *Lauraceae*, and *Hernandiaceae* (Fig. 79) the nectaries are modified stamens.

In contrast with the *Laurales*, the *Ranunculaceae* and *Berberidaceae* represent a development which has been largely hypogenous. In the *Ranunculaceae*, the nectaries, with few exceptions, are very evidently modified stamens. Often the outermost of each row of stamens is a modified nectar gland. Where there are petals these are very evidently honey leaves

which are simply modified honey glands. The development of petals in this manner is very clearly indicated in the genus *Ranunculus*.

In the *Berberidaceae* and *Menispermaceae*, order *Berberidales*, there are often two series of honey leaves exterior to the stamens. These families have usually been considered as close to the *Ranunculaceae*. The only logical explanation of their honey leaves would seem to be that these are structures similar to the honey leaves of the *Ranunculaceae*. The *Berberidales* would appear to represent a more woody and the *Ranunculales* a more herbaceous development from a common ancestry. Before leaving this subject, it may be well to point out that the honey glands in the *Laurales* may be very similar to those of the *Ranunculaceae*. The very peculiar nectaries of *Helleborus* are duplicated in *Illigera* (Fig. 79) of the *Hernandiaceae*, order *Laurales*.

The petals of *Nymphaea advena* afford strong evidence for the inclusion of *Nymphaeaceae* in or near *Ranunculales*. Here the petals are small inconspicuous structures, but they are honey glands which secrete nectar abundantly on their lower surfaces. They are very similar to the nectaries or honey leaves of the *Ranunculaceae*. According to Müller (1) a similar condition is found in *Nymphaea lutem*.

Geraniales

This order has been regarded as unique in that the androecium is obdiplostemonous; that is, that the innermost row of stamens is opposite the petals instead of alternating with them. In other words, there seems to be a whorl of stamens lacking. An examination of the nectaries may be of some interest.

The general situation in *Geranium* is that there are ten stamens arranged in an inner and outer series. Attached to the torus below and outside of each of the stamens of the inner series is a conspicuous gland which projects out from the torus. These glands occupy the position in which one would expect to find a third series of stamens, if such existed. The position and character of these nectar glands is such that they may be modified stamens.

The only group of plants we have considered in which individual stamens have become individual glands are the *Laurales*, *Berberidales*, and *Ranunculales*. In the *Ranunculaceae* it is very common for the outermost members of a row of stamens to be nectariferous.

Alternating with the nectar glands in *Geranium* are five petals. If *Geraniales* are related to the *Ranunculaceae*, then it would seem that the petals would also be modified honey leaves derived from stamens, as all petals in *Ranunculaceae* appear to have had this origin. I know of no direct evidence that the petals of *Geraniales* are modified honey leaves, but in *Delphinium* there are five petals all of which were probably honey leaves. Two of these retain the function of secreting nectar while three are petals.

In, at least, some species of *Geranium* the petals arch up and enclose the nectaries so that entrance to them is by means of an opening between two adjacent petals. If *Geranium* is derived from *Ranunculales*, the interpretation would seem to be that there were originally ten stamens which became nectar glands, that five of these retained that function, while five have been transformed into petals. If the nectar glands are modified stamens, then the flowers are not really obdiplostemonous. The outer row of stamens modified as nectar glands would alternate with the petals.

In *Oxalidaceae* there are, usually, ten stamens which consist of an inner whorl of five alternating with an outer whorl of five. The bases of all the stamens are united to form a short tube. In *Biophytum sensitivum* (Fig. 80) there is a conspicuous, oval, nectar gland on the outside at the base of each of the shorter stamens. In *Oxalis* and *Averrhoa* (Fig. 81) there is a nectariferous region in the lower part of each of the shorter stamens. The condition in the *Oxalicadeae* would be similar to that of *Geranium* if we assume that in *Biophytum*, in the general fusion of the basis of the stamens, the stamens modified as nectar glands have become attached to the ring. In *Oxalis* and *Averrhoa* the fusion would simply be more complete.

In *Linum usitatissimum* (*Linaceae*) the nectaries are capable of a similar interpretation. Here there are only five

stamens and sometimes small protuberances representing the lost whorl. The nectar glands are attached to the bases of the functional stamens (Fig. 82).

In *Tribulus* (*Zygophyllaceae*) the nectary can be interpreted along similar lines. There is nectariferous tissue all around the bases of the ten stamens (Fig. 83). All of this is attached to the torus. There is, however, a very conspicuous projection below the base of each stamen of the innermost whorl. This projection is in the position of the nectaries in *Geranium*, and it is this projection that exudes most of the nectar. The latter is protected by the petals as in *Geranium*.

The discussion above brings out the fact that there is a great deal of similarity in the nectaries of the members of the *Geraniales* with regular flowers. The nectaries also suggest a possibility of an alliance of the *Geraniales* with the *Ranunculales*. It must be said, however, that the evidence is rather speculative.

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EXPLANATION OF PLATES

PLATE I

FIG. 1. *Magnolia coco* (Lour.) DC.
FIG. 2. *Haworthia reinwardtii*. Longitudinal section, showing septal gland (*N*).
FIG. 3. *Haworthia reinwardtii*. Cross section of ovary, showing septal gland (*N*).
FIG. 4. *Cocos nucifera* L. Ovary showing openings of septal glands (*N*).
FIG. 5. *Cocos nucifera* L. Cross section of ovary showing septal glands (*N*).
FIG. 6. *Fourcroya*. Longitudinal section of flower showing septal gland (*N*).
FIG. 7. *Gladiolus*. Longitudinal section of flower showing septal gland (*N*).
FIG. 8. *Gladiolus*. Cross section of ovary showing septal glands (*N*).
FIG. 9. *Narcissus*. Longitudinal section of flower showing septal gland (*N*).

PLATE I

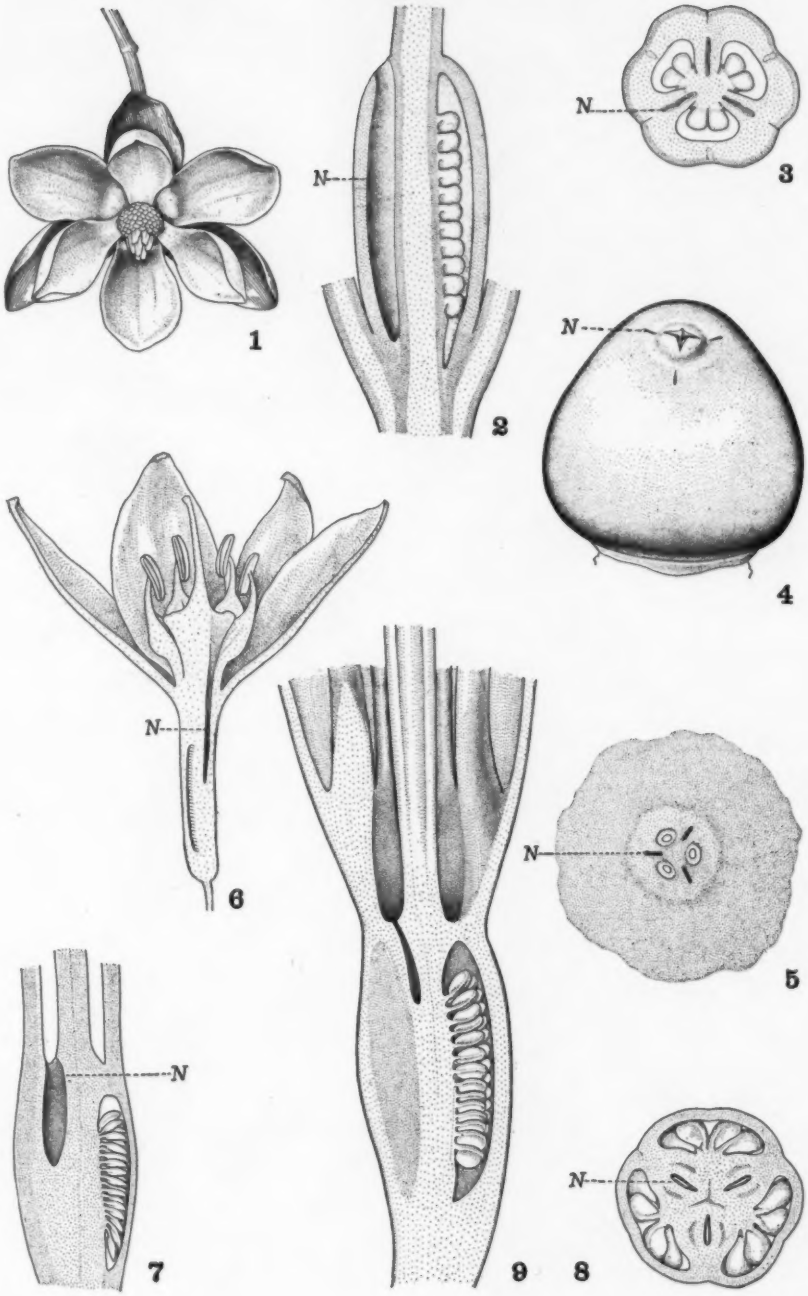


PLATE II

FIG. 10. *Tillandsia*. Longitudinal section of ovary showing septal gland (*N*).

FIG. 11. *Ananas comosus* Merr. Cross section of portion of flowering head showing septal gland (*N*).

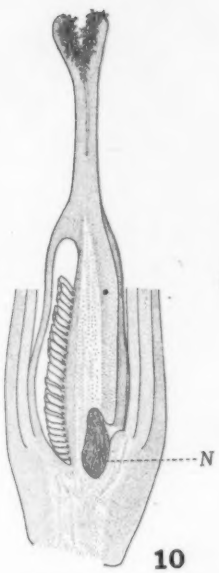
FIG. 12. *Billbergia*. Longitudinal section of lower part of flower showing septal gland (*N*).

FIG. 13. *Costus speciosus* (Koen.) Sm. Section of lower part of flower showing septal gland (*N*).

FIG. 14. *Alpinia speciosa*. Lower part of flower with half of perianth removed to show nectary (*N*).

FIG. 15. *Belamcandia chinensis* (L.) DC. Central portion of flower showing nectar glands (*N*).

PLATE II



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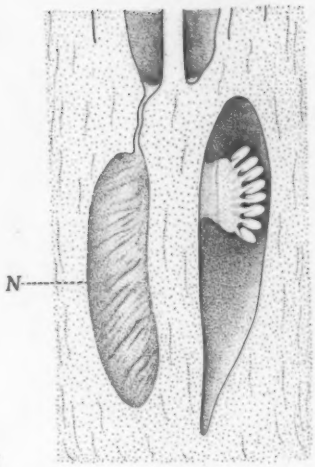

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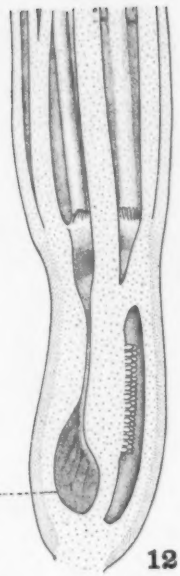

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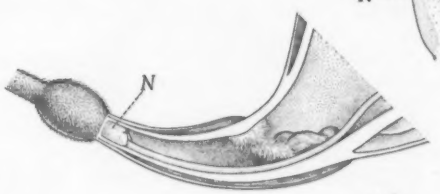

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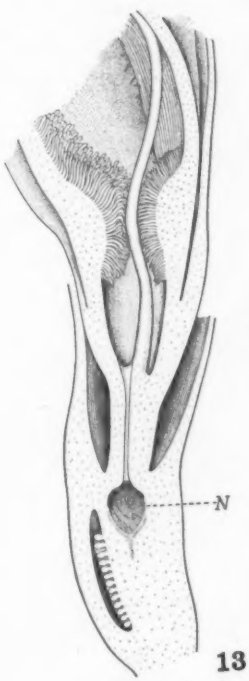

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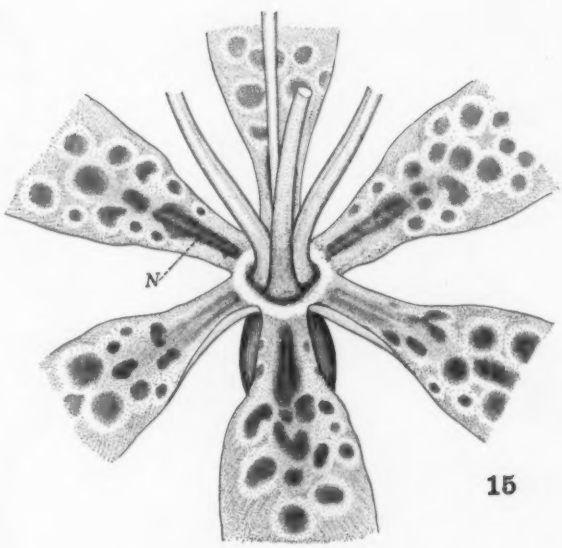

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PLATE III

FIG. 16. *Lilium*. Perianth segment showing groove in which nectar is secreted (*N*).

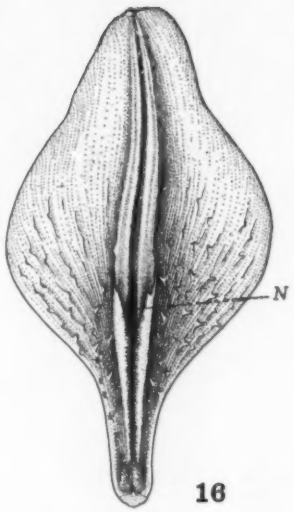
FIG. 17. *Sagittaria sagittaefolia* L. Longitudinal section through portion of female flower showing nectaries between carpel and staminode and between staminode and petal (*N*). × 30.

FIG. 18. *Sagittaria sagittaefolia* L. Section through rudimentary carpels of male flower to show nectaries (*N*).

FIG. 19. *Livistona*. Section through carpels to show nectary (*N*).

FIG. 20. *Livistona*. Section through nectariferous cells between basal parts of carpels (*N*). × 120.

PLATE III



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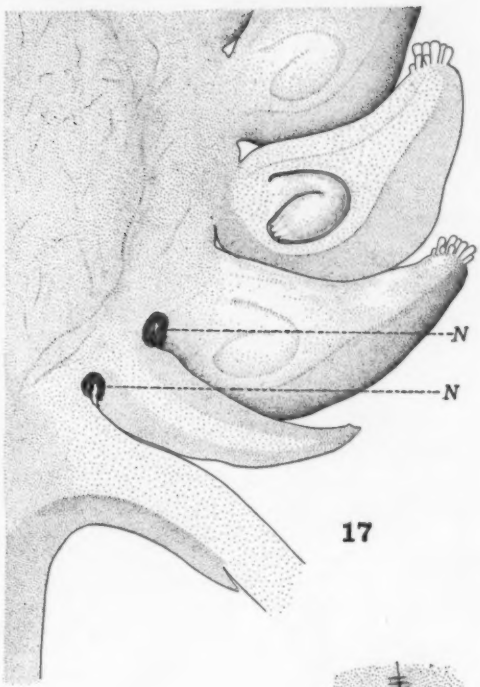

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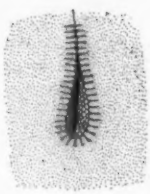

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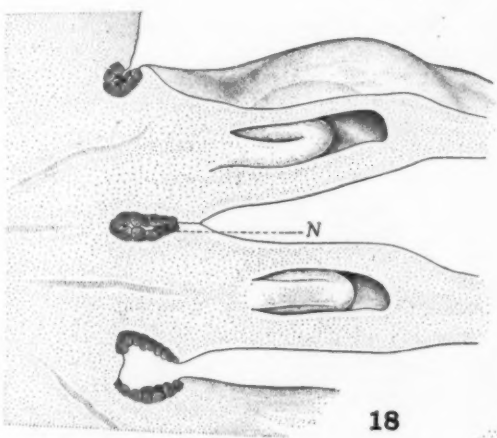

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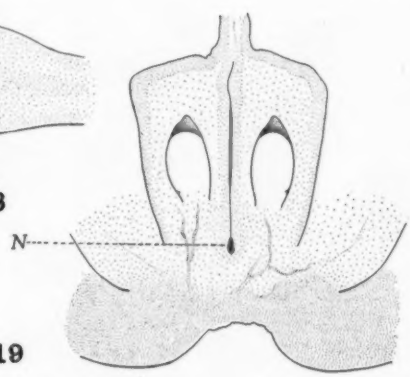

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PLATE IV

FIG. 21. *Tea sinensis* L. Section through central part of flower to show nectariferous region. (*N*).

FIG. 22. *Eurya japonica* Thunb. Section of ovary to show nectary (*N*).

FIG. 23. *Clethra lancifolia* Turcz. Flower showing position of ovary and stamens (*N*).

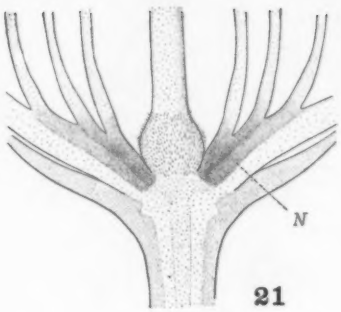
FIG. 24. *Clethra lancifolia* Turcz. Section of central part of flower to show nectary around base of ovary (*N*).

FIG. 25. *Rhododendron williamsii* Merr. Section showing nectary (*N*) not attached to ovary.

FIG. 26. *Azalea japonica* Gray. Section of central part of flower to show that nectary (*N*) is part of ovary wall.

FIG. 27. *Ehretia navesii* Vid. Section of flower to show that nectary (*N*) is around basal part of ovary.

PLATE IV



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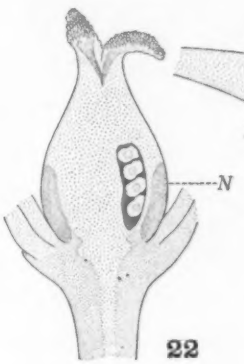

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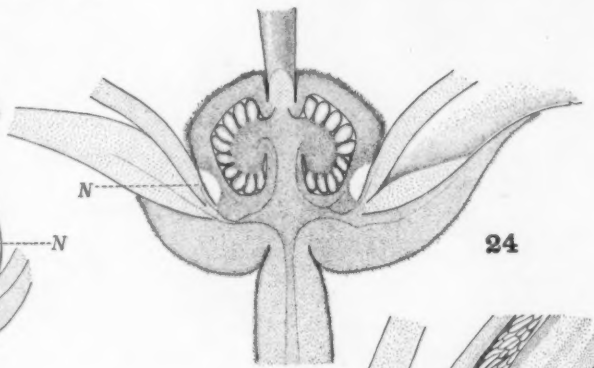

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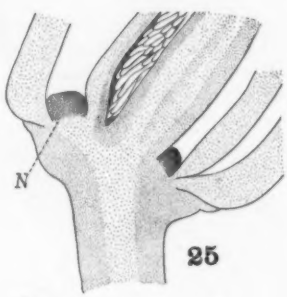

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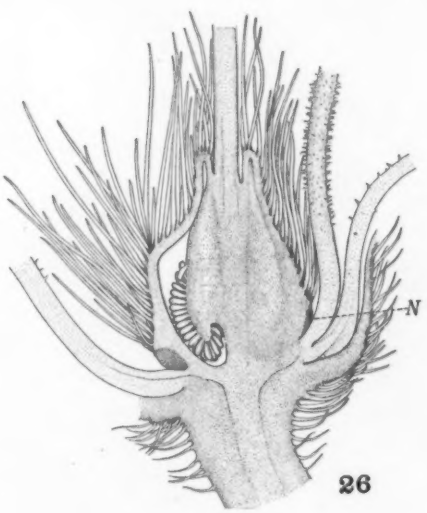

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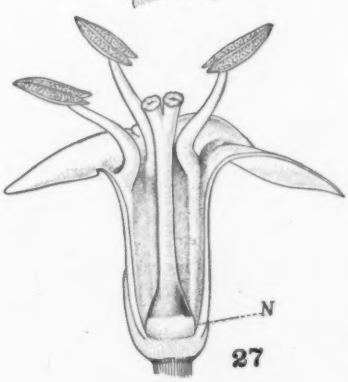

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PLATE V

FIG. 28. *Phlox drummondii* Hook. Section of flower showing that nectary (*N*) is around base of ovary.

FIG. 29. *Tectona grandis* L.f. Section of ovary showing nectary (*N*) around base of ovary.

FIG. 30. *Gypsophita*. Section of flower showing nectary (*N*) as a ring below stamens.

FIG. 31. *Portulaca*. Section of central part of flower showing tissue (*N*) sucked by insects as a ring below the stamens.

FIG. 32. *Coccoloba uvifera*. Section of flower showing nectary as a ring below the stamens.

FIG. 33. *Basella rubra* L. Section of flower showing nectary as a ring below the stamens.

PLATE V

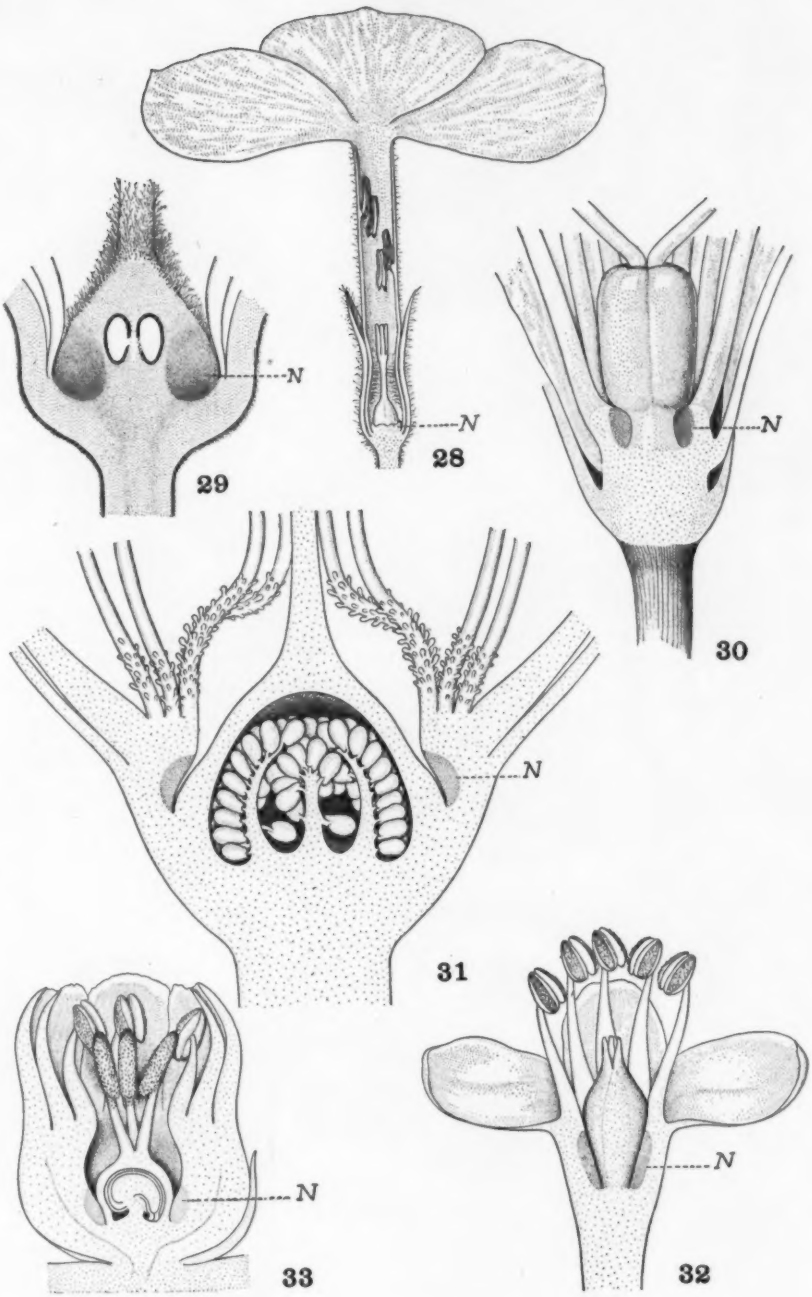


PLATE VI

FIG. 34. *Avicennia officinalis* L. Section of flower showing nectariferous corolla (*N*).

FIG. 35. *Corchorus obitorius* L. Section of central part of flower showing nectary (*N*) consisting of a patch of glandular hairs.

FIG. 36. *Gossypium herbaceum* L. Section through nectary (*N*).

FIG. 37. *Gossypium herbaceum* L. Corolla showing slits through which the nectar is made available (*N*).

FIG. 38. *Helicteres hirsuta* Lour. Flower.

FIG. 39. *Helicteres hirsuta* Lour. Flower with half of calyx removed.

FIG. 40. Cherry. Section of flower showing nectary disc (*N*).

FIG. 41. Raspberry. Flower showing nectary disc (*N*).

PLATE VI

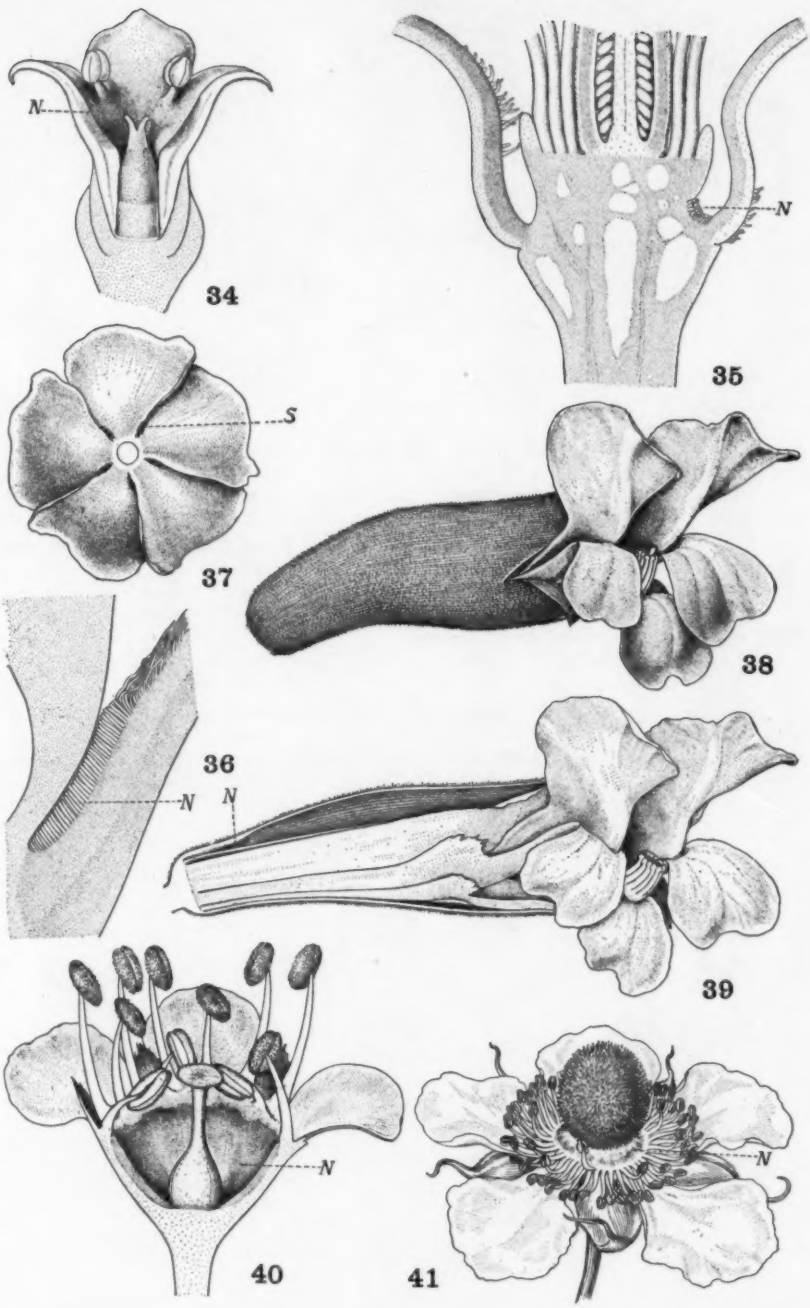


PLATE VII

FIG. 42. *Deutzia pulchra* Vid. Section of flower to show nectary (*N*) disc above inferior ovary.

FIG. 43. *Gymnosporia spinosa* (Blm.) Merr. and Rolfe. Section of ovary surrounded by nectary disc (*N*).

FIG. 44. *Perrottetia alpestris* (Blm.) Loesen. Longitudinal section of flower to show the position of nectary disc (*N*) between ovary and stamens.

FIG. 45. *Zizyphus jujuba* (L.) Lam. Flower showing nectary disc (*N*).

FIG. 46. *Strombosia philippinensis* (Baill.) Rolfe. Section of flower to show nectary disc around the ovary (*N*).

FIG. 47. *Nephelium lappaceum* L. Section of flower to show position of nectary (*N*).

FIG. 48. *Evodia confusa* Merr. Section of central portion of flower to show nectary around ovary (*N*).

FIG. 49. *Citrus decumana* Murr. Section of flower to show nectary around base of ovary (*N*).

PLATE VII

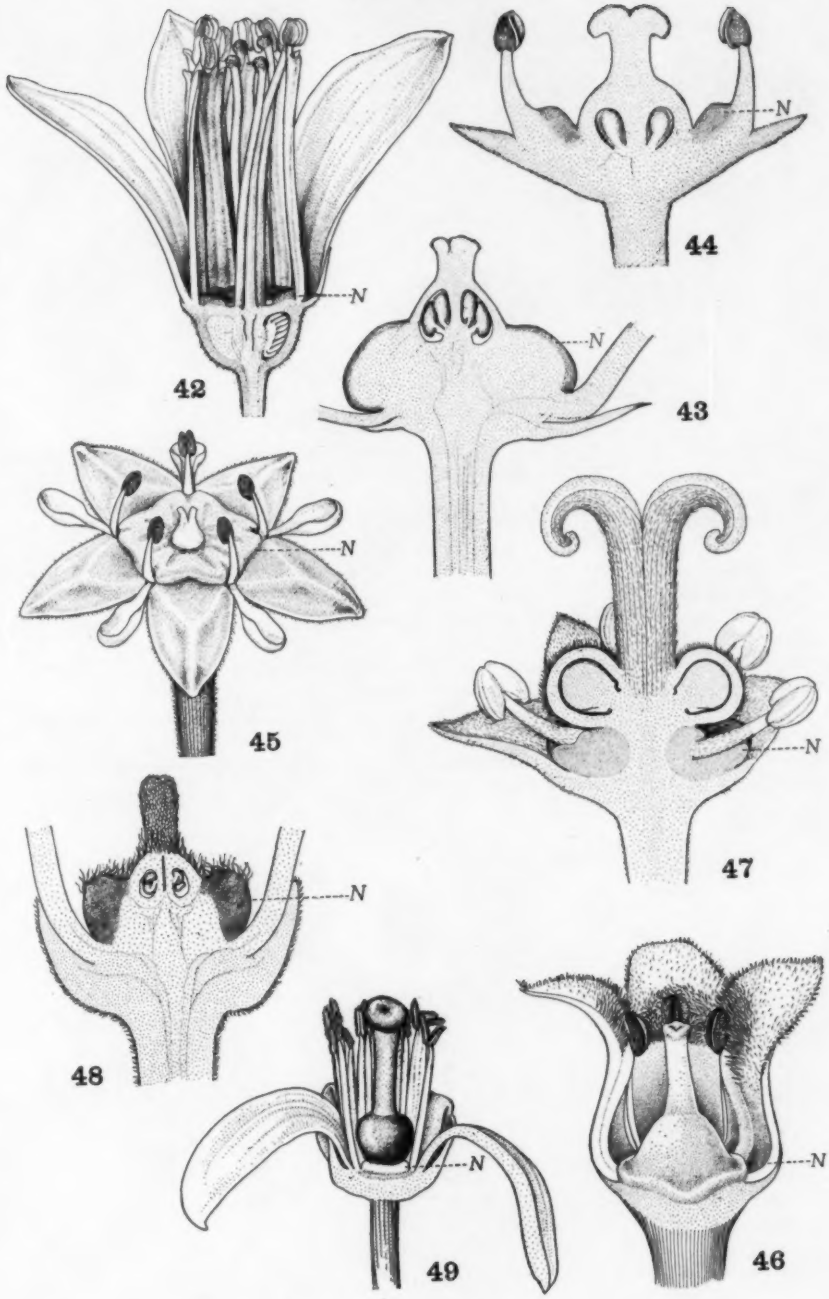


PLATE VIII

FIG. 50. *Melia azedarach* L. Section of flower to show position of nectary.

FIG. 51. *Bridelia stipularis* (L.) Blm. Section of male flower to show wide nectary disc (*N*).

FIG. 52. *Spondias purpurea* L. Section of central portion of flower to show a large nectary disc (*N*) attached to torus.

FIG. 53. *Jatropha curcas* L. Section of flower to show large nectary disc. Consists of five lobes (*N*).

FIG. 54. *Anethum graveolens* L. Flower showing nectary disc above the ovary (*N*).

FIG. 55. *Mastixia philippinensis* Wang. Longitudinal section of flower to show position of nectary above an inferior ovary (*N*).

FIG. 56. *Nothopanax guilfoylei* (Cogn. & March.) Merr. Section through flower which shows position of nectary disc (*N*) above inferior ovary.

FIG. 57. *Viburnum odoratissimum* Ker. Section through flower showing position of nectary disc (*N*) above inferior ovary.

PLATE VIII

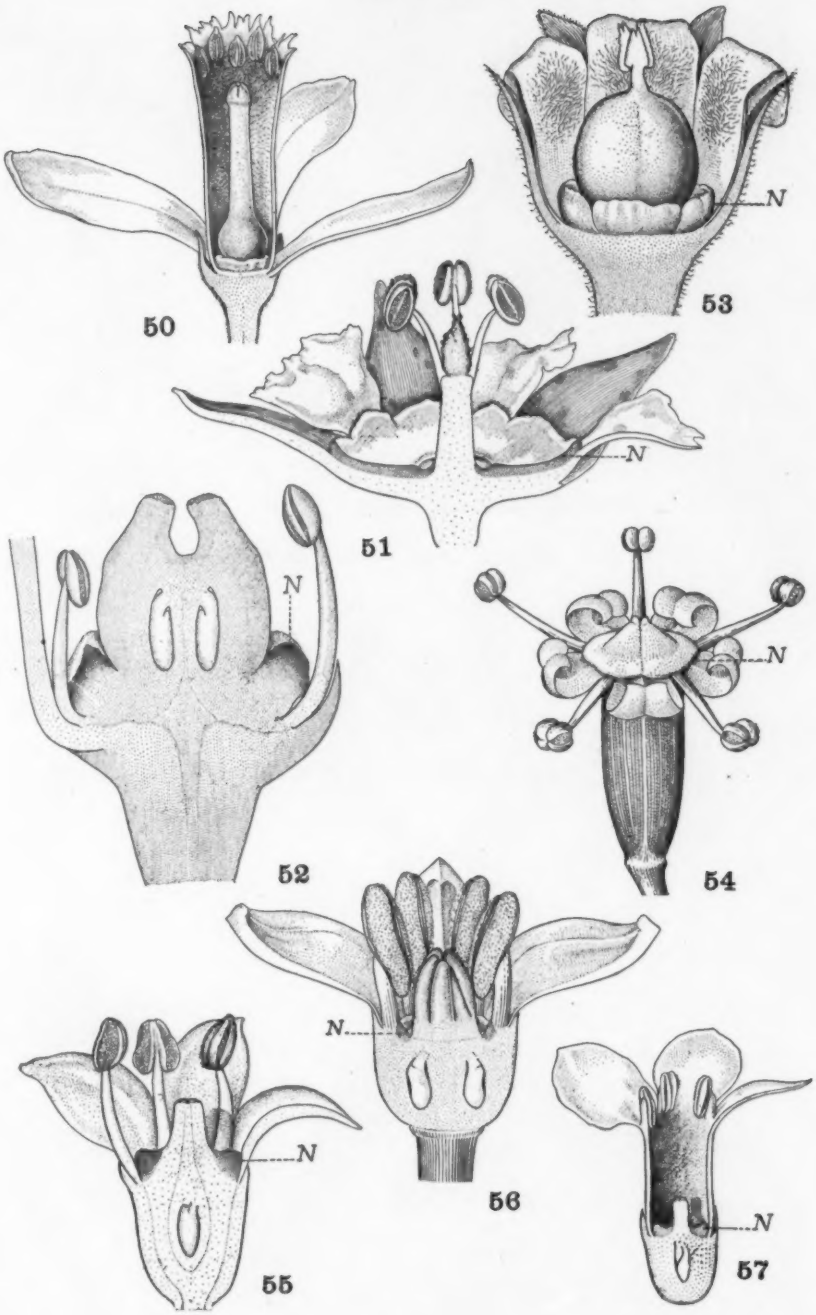


PLATE IX

FIG. 58. *Pavetta.* Section of flower showing a nectary disc above inferior ovary.

FIG. 59. *Campanula medium* L. Section of flower showing nectary disc (*N*) above inferior ovary.

FIG. 60. *Tithonia speciosa.* Longitudinal section of flower showing nectary tube around base of style (*N*).

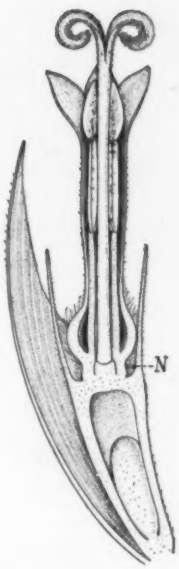
FIG. 61. *Solidago.* Longitudinal section of flower showing nectary tube around base of style (*N*).

FIG. 62. *Uncaria philippinensis* Elm. Longitudinal section of basal portion of flower to show nectary disc attached to both torus and corolla tube (*N*).

FIG. 63. *Wikstroemia meyeniana* Warb. Section of basal portion of flower showing nectary disc attached to torus around the ovary (*N*).

FIG. 64. *Eugenia malaccensis* L. Longitudinal section of flower showing nectary lining the cup above the inferior ovary (*N*).

PLATE IX



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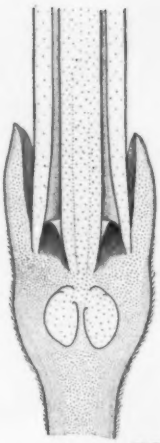

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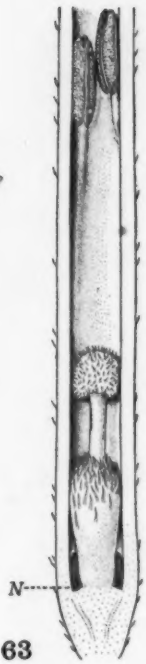

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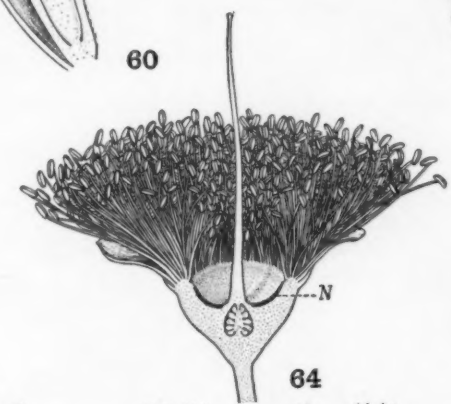

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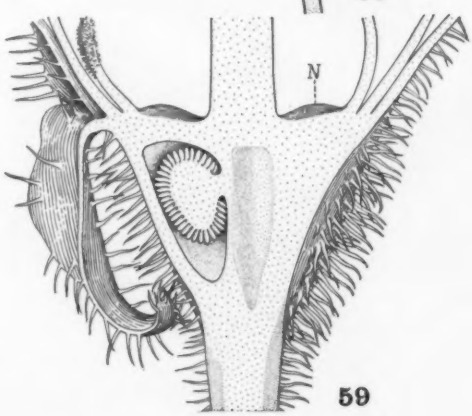

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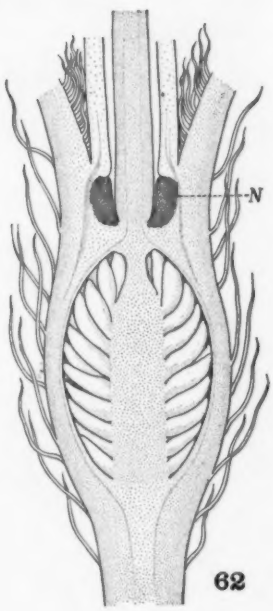

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Plate X

Fig. 65. *Gynothroches axillaris* Blm. Section of flower showing position of nectary disc (*N*).

Fig. 66. *Combretum squamosum* Roxb. Section of flower showing nectary disc lining cup above the ovary (*N*).

Fig. 67. *Eucalyptus corymbosa*. Section of flower showing nectary disc lining cup above the ovary (*N*).

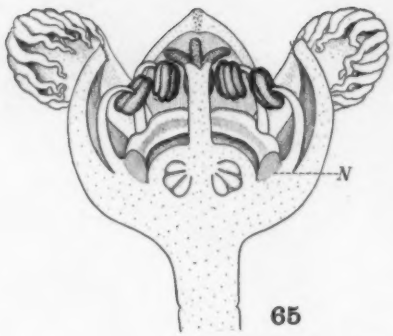
Fig. 68. *Oenothera lamarckiana* Ser. Longitudinal section of basal portion of flower showing location of nectary disc (*N*).

Fig. 69. *Cnestis palala* (Lour.) Merr. Longitudinal section of flower showing nectary (*N*).

Fig. 70. *Bixa orellana* L. Longitudinal section of flower showing nectary disc to which stamens are attached (*N*).

Fig. 71. *Scolopia luzonensis* (Presl.) Warb. Longitudinal section of flower showing nectary disc to which stamens are attached (*N*).

PLATE X



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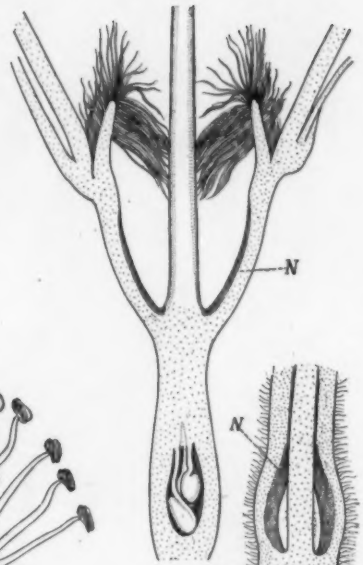

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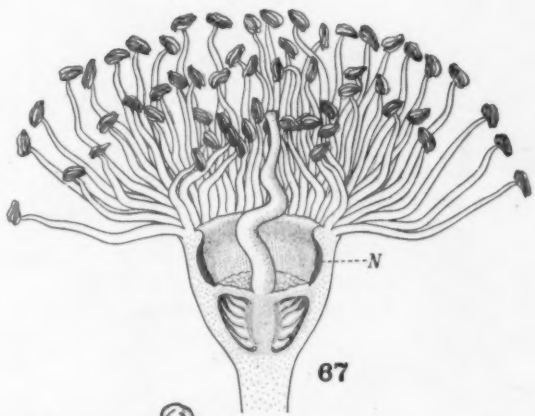

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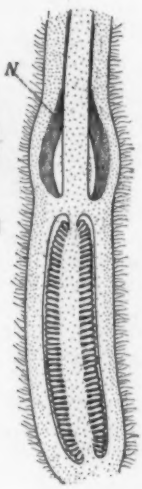

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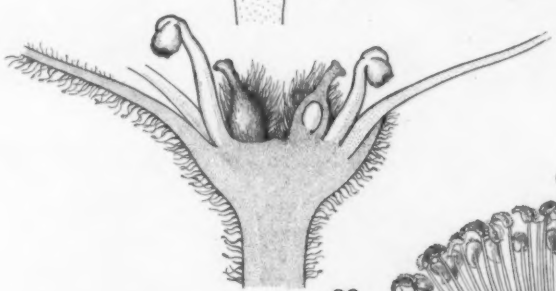

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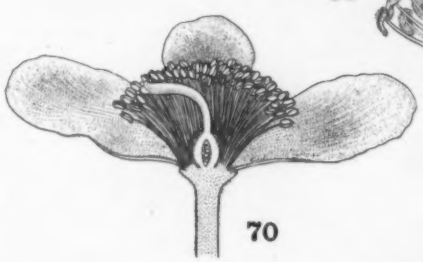

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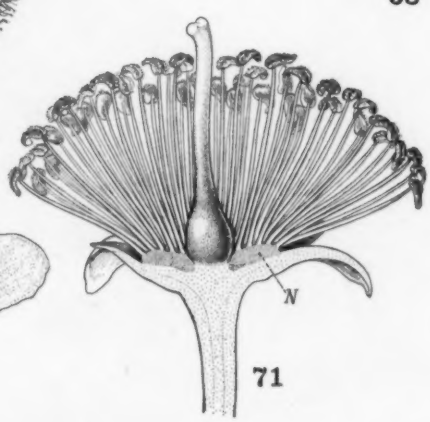

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PLATE XI

FIG. 72. *Flacourtia rukam* Zoll. & Morr. A flower showing disc around base of stamens (*N*).

FIG. 73. *Ahernia glandulosa* Merr. Central portion of flower showing nectary in the form of lobes interspersed among the stamens (*N*).

FIG. 74. *Capparis lobbiana* Turcz. Basal portion of flower with petals and calyx removed to show nectary disc to which stamens are attached (*N*).

FIG. 75. *Crataeva religiosa* Forst. Flowers showing nectary disc in form of cup around the stamens (*N*).

FIG. 76. *Gynandropsis gynandra* (L.) Merr. Center of flower showing nectary (*N*).

PLATE XI

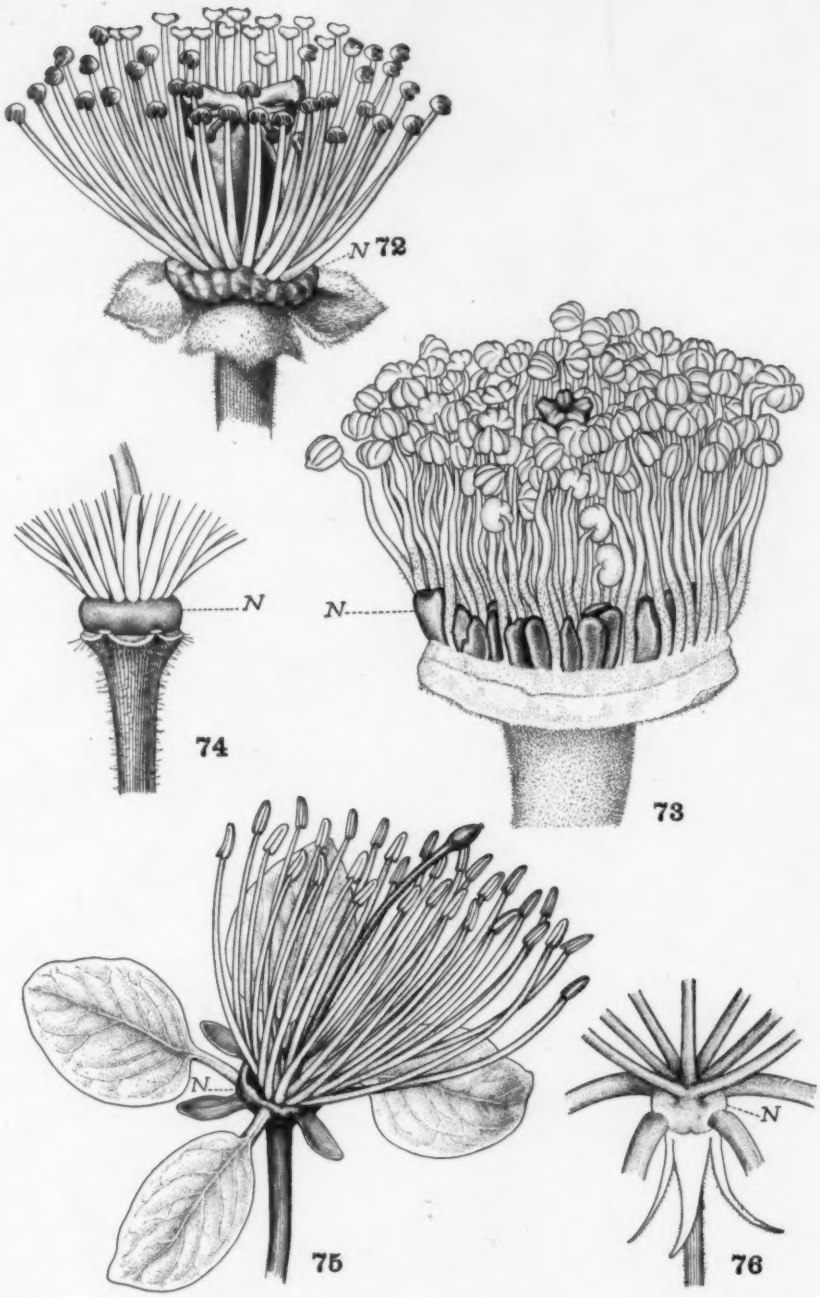

Plate XII

Fig. 77. *Cinnamomum camphora* Nees & Eberm. Flower showing nectaries.

Fig. 78. *Cinnamomum camphora* Nees & Eberm. Longitudinal section of flower.

Fig. 79. *Illigera luzonensis* (Presl.) Merr. Flower showing nectary glands (*N*)

Fig. 80. *Biophytum sensitivum* (L.) DC. Section of flower showing nectary glands in the bases of short stamens (*N*).

Fig. 81. *Averrhoa carombola* L. Section of flower.

Fig. 82. *Linum usitatissimum* L. Ovaries surrounded by staminal ring and with nectar glands below the bases of the stamens (*N*).

Fig. 83. *Tribulus cistoides* L. Ovary with the base surrounded by the nectary. In the nectary are the scars of the stamens and petals (*N*).

PLATE XII

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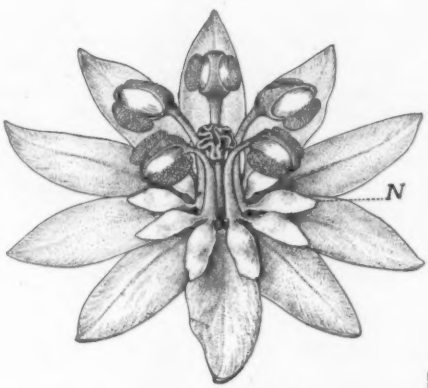

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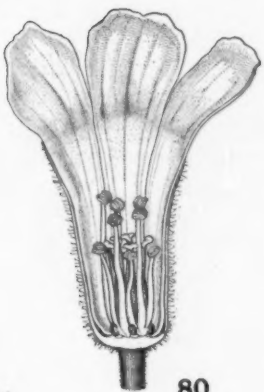

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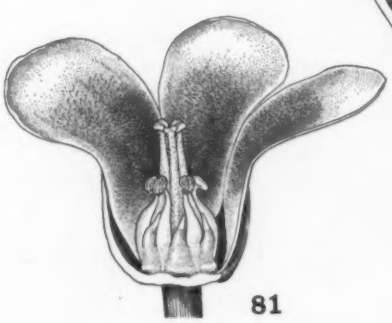

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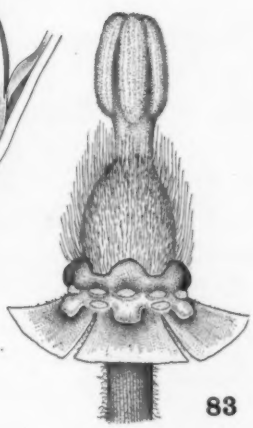

83

THE ORIGIN OF THE GRASPING MOVEMENT IN MAN [1]

DAVENPORT HOOKER

Professor of Anatomy, University of Pittsburgh School of Medicine

(Communicated by Edwin G. Conklin)

(*Read April 21, 1938*)

ABSTRACT

Evidence is presented indicating that the grasping reflex first appears as a partial finger flexion, in which the thumb is not involved, at about the 11th week of menstrual age in human fetuses. Finger flexion becomes quite complete by 13½ to 14 weeks. Gripping an object appears about the 18th week. Though the thumb may be feebly motile as early as 12 weeks, in response to palmar tactile stimulation, it rarely responds with regularity before the 15th week, and does not play any role in the grasp by 25 weeks, the terminal age of these observations. Further observation may alter somewhat the age levels at which these characteristic responses appear. Furthermore, the conditions under which the observations are made, involving slow, progressive asphyxia, necessitate care in generalization.

INTRODUCTION

THE postnatal development of the grasping reflex has been studied in detail. Though evidence is available which indicates that this reflex has a prenatal origin, no systemic study of it is present in the literature. Indeed, the observations of prenatal grasping so far recorded are both few and scattered, limited largely to the classic observations of Minkowski (1920 et seq.) and to the report of Bolaffio and Artom (1924).

Minkowski (1928) states that stroking the palm frequently caused flexion of the fingers, and flexion, abduction and, sometimes, opposition of the thumb. This resulted in a definite grasping reflex, in which the hand closed into a fist, at times. Such a reflex he observed in a fetus of 65 mm. total length. This represents a menstrual age of about 11

[1] Supported by a grant from the Penrose Fund of the American Philosophical Society. Technical assistance, made possible by the grant, has been rendered by Reinhardt Rosenberg.

weeks, and was presumably the earliest age at which the reflex was observed by him. Minkowski reports that he had observed the closed fist response on palm stroking until at least six months of gestational age. It is unfortunate that Minkowski gives no definite information regarding the strength of the stimulus applied to secure responses, merely stating (1921) that he used "light touching of the skin by means of a brush, friction, pressure, etc." He does, however, give evidence that, in fetuses of 65 and 160 mm. total length, the response to palmar stimulation was a true reflex.

Bolaffio and Artom (1924) observed the reactions of a number of human fetuses to a variety of mechanical and electrical stimuli. Unfortunately, again, information regarding details of the nature of the stimulus applied to the palm is lacking. Furthermore, these authors offer no such proof of the reflex nature of the response, as is afforded by Minkowski. They report that a 70 mm. fetus (about 12 weeks of menstrual age), when stimulated by "gentle touching or percussion," showed "slow local contractions of all muscles of the extremities," but they do not specifically mention finger closure. However, they observed grasping reflexes in fetuses older than 6 months, though only on relatively deep pressure stimulation of the palm.

Since the autumn of 1932, it has been my privilege to observe the activities of a number of fetuses and to record cinematographically a considerable number of reflexes. These observations are, as yet, by no means complete, because the cases available are dependent upon the misfortune of premature termination of pregnancy and, hence, the age distribution is entirely a matter of chance. Furthermore, all observations made so far have been on younger age levels, none of which is capable of existence beyond a few minutes or, at most (in the slightly older cases) a few hours. The time available for observation is, therefore, short. Nevertheless, observations made on the grasping reflex now cover a fairly comprehensive age series. As these give somewhat more definite evidence on the time of appearance of this reflex and

on its development in fetal life than has hitherto been available, this preliminary report is presented.

Hand Posture

The individual digits appear in the human hand about $7\frac{1}{2}$ weeks of age, as reckoned from the beginning of the previous menstrual period (menstrual age). By about 8 weeks, the thumb, originally in line with the other fingers as they develop from the flat spatulate enlargement of the primitive hand, has begun its rotation toward the adult position. This it seems to attain, morphologically, before 11 weeks, though, as shown by Gesell and Halverson (1936), it is incapable of effective opposition much before the 24th week after birth.

The posture of the hands during early fetal life varies with different age levels. At 8 to $8\frac{1}{2}$ weeks of menstrual age, the hands of a living fetus, in fluid, are held in a prone position, somewhat pendent from the forearms, which lie parallel to the sagittal plane of the body. The fingers are only slightly bent; the forearms are a little more than half flexed on the arms. When the fetus is out of fluid or has been fixed, the hands are folded on the chest. This position is often described and figured as the normal posture. As the age of the fetus increases, the palms tend to face more medially and the hands to approach one another. The fingers are observed to be progressively more flexed in posture. By about $13\frac{1}{2}$ weeks, the fingers are held almost perpendicular to the palm; by 20 weeks, they tend to be quite fully flexed, so that their tips touch the palm, a characteristic finger posture during the remainder of fetal life and for a short time after birth. From about $13\frac{1}{2}$ to 16 weeks of menstrual age, the hands are frequently opened and closed spontaneously, but, after this age, the fingers are ordinarily extended only momentarily and at long intervals.

Methods and Materials

In the observations here presented, the stimulus used to produce finger flexion was a short hair with a beaded tip.

A series of such stimulators, graded so as to be capable of exerting, respectively, maximum pressures of 10, 25, 50, and 100 milligrams, and of 2 grams, were used (see Hooker, 1936). The 10 mg. stimulator was generally ineffectual, the 25 mg. hair being necessary (though not at its maximum pressure) to cause a tactile stimulation of the palm in the younger cases. The pressure required to produce such stimulation increased with age. After about 15 weeks of menstrual age, the 2 gram hair, frequently also used on slightly younger cases, was necessary to evoke a response. Occasionally, higher pressure hairs were used in young cases to determine the presence of direct muscle irritability. Glass rods were used to test gripping.

Observations have been made on a total of 39 fetuses ranging in menstrual age from 7 to 25 weeks.[2] Of this number, 17 failed to exhibit any response to stimulation of the palm, either because of lack of development (13) or for other reasons (4).

Observations

Fetuses of 9½ weeks of menstrual age did not give any response to tactile stimulation of the palm, though, in one instance, very slight finger flexion was observed after prolonged palmar stimulation with a pressure of about 2 grams. The facts that movement also occurred at wrist, elbow and shoulder in this individual, and that lighter hairs failed to evoke such a reflex at this age in every other case, seem to indicate that this particular response was due to direct mechanical stimulation and was not a true reflex. Only one 10 weeks fetus has been observed and it did not exhibit response to palmar stimulation. Absence of the grasping reflex may not be significant, since it also failed to give other responses normally present at 9½ weeks.

However, when the palm of a living 11 weeks fetus was stroked with a light hair, the fingers flexed, but the thumb

[2] The age distribution of the 39 fetuses is as follows, the number observed at any age being in parentheses following the number of weeks of menstrual age: 7(1), 7½(2), 8(1), 8½(5), 9½(4), 10(1), 11(1), 11½(3), 12(2), 12½(1), 13(1), 13½(1), 14(5), 14½(1), 15(1), 15½(1), 16(1), 17(1), 17½(1), 18½(2), 19½(1), 22(1), 25(1).

did not. The flexion was extremely rapid. The reaction posture persisted for less than 1/5 second, the fingers then relaxing and returning to their normal posture. The amount of finger flexion was slight, as proved by careful examination of the individual frames of the cinematographic record. The movement appears to have been limited to the phalangeal joints. If movement occurred at the metacarpophalangeal joints, it was so slight as to defy measurement. Minkowski (1928) reported that a fetus of about this age formed a fist. Such was also the impression of the observers who witnessed the original responses in this case. However, examination of each record of the several responses secured has proved that finger flexion was far from complete. The rapidity of the flexion created a false impression of its extent. Similar results were secured with the 11½ weeks cases observed.

Human fetal reactions mature with great celerity. At 12 weeks, a very fair "fist" may often be made on light stimulation of the palm. Here, as at 11 weeks, the response is stereotyped, independent of the region of the palm stimulated, and the finger flexion is rarely complete. By 13½ weeks, the hand may be opened and closed spontaneously with an ease of motion quite in contrast to the response to tactile stimulation at earlier age levels, though the tactile response, even through 13½ weeks, maintains something of the stereotyped character. Finger closure may be quite complete at these ages.

Spontaneous movements of the hands begin at about 13½ weeks of menstrual age, approximately 5 weeks after human fetuses begin to exhibit any responses to tactile stimulation and 4 after the first spontaneous movements have been observed. All spontaneous movements prior to 13½ weeks of menstrual age appear less stereotyped than responses to tactile stimulation, possibly because several centers, rather than one, are activated at the same time. In spite of their seeming freedom as movements, fewer types of activity appear spontaneously than can be elicited by tactile stimulation, at any of these age levels.

Beginning at about 13½ weeks, stimulation of the palm frequently caused movement of the forearm at the elbow and shoulder, in addition to finger flexion. The direction of this movement of the forearm might be in any plane, though it was predominantly upward or downward. Superficially observed, the hand appeared to be following the stimulus. Analysis of the cinematographic records indicates no connection between the area of the palm stimulated or the direction of the stimulating stroke with the direction in which the hand was moved. Though further observation of this movement may demonstrate a connection, at present the direction of movement of the hand appears to be without significance.

Beginning about 16 weeks of menstrual age, but varying in individual cases, all responses of human fetuses tend to become less active and, in general, more difficult to elicit. The cause of this, to which there are occasional marked exceptions, is not definitely known, but it is believed that certain profound changes occur at about this period which alter the oxygen supply of the fetal tissues. This increased threshold, coupled with the normal posture of closed fingers, makes testing for finger flexion difficult in this later period. When the fingers are fully flexed, stimulation of the "heel" of the palm (the only area uncovered) tends to cause tightening of the fingers and some wrist flexion. However, when the fingers can be found extended, palmar stimulation causes complete finger closure.

The thumb may become feebly motile at about 12 weeks but rarely responds with any regularity before 15 weeks and plays no active role in the fetal grasping reflex at any age up to 25 weeks. This is also the condition after birth, as shown by Gesell and Halverson (1936). The fetal thumb is frequently caught under the forefinger and sometimes under the third digit as well, so that an appearance of a fist formation, in which the thumb participates, is simulated. Though its motility increases with age, its opposition has not been observed in any case and it never performs a true function in grasping until some time after birth.

During the earlier development of finger flexion, no evidence was found to indicate that the fingers were clinging to the object in contact with the palm. The reaction seemed to be a relatively simple reflex. At about the 18th to 20th week, however, the fingers tended to remain closed, if the stimulator were left in contact with the palm. By 20 weeks of menstrual age, the closed fingers seemed to grasp feebly a thin, smooth glass rod placed within them. At 22 weeks, the grip on the rod was sufficient to enable the observer to move the entire arm over a short distance by gentle pulling in the direction of the long axis of the rod. Though the grip became progressively stronger with increased age, it was never strong enough to seriously resist withdrawal of the rod, even at 25 weeks, the terminal age of these observations. At this age, however, a temporarily resuscitated fetus has been observed to grasp a glass rod, weighing $3\frac{1}{2}$ grams, with sufficient grip to retain it as the arms were moved about.

Discussion

Incomplete though it is, the evidence presented in this report demonstrates that the grasping reflex in man has a definite developmental history extending into relatively early fetal life. It first appears as an incomplete finger flexion response at about 11 weeks of menstrual age. The observations indicate that the amount of finger flexion increases with age. At $13\frac{1}{2}$ weeks, it may be quite complete. By 16 weeks, the fingers almost always flex completely. So far as the evidence shows, the closed fingers do not tend to grip the stimulator before an age of about 18 weeks. The capacity of the fingers to cling to an object develops progressively, also, though relatively little strength of grip is exhibited within the age span studied. The successive appearance and development of finger flexion and of gripping lends support to the statement of Halverson (1937), that "the grasping reflex is a two-phase activity consisting of finger closure and gripping (clinging)."

In general, the results of the observations made in this study agree with those of Minkowski, though they provide a

somewhat more fully detailed sequence. There are, however, a few points of difference, either in observation or interpretation. Minkowski did not have the advantage of being able to study cinematographic records of the reactions observed by him, whereas such records formed the basis of this investigation. The disagreement regarding the amount of finger closure at the time of earliest appearance is, therefore, probably without significance other than to prove the value of cinematographic recording. As a matter of fact, the total length of Minkowski's earliest case (65 mm.) was less than the recorded crown-heel length (68 mm.) of the 11 weeks fetus reported in this study. Very slight differences in the position of the head and lower extremities will readily account for much greater variations in measurement, so that Minkowski's case may, or may not, have been younger. It should be noted that no reliable observations were made, in this study, on fetuses between $9\frac{1}{2}$ and 11 weeks of age. Future observations may carry the first appearance of finger closure to a slightly earlier age level.

Two other considerations should also be kept in mind in any endeavor to establish the time of appearance of any biological phenomenon such as this. In the first place, a particular activity will not be observed at the same chronological age in all individuals, as the time at which it can appear is dependent upon the developmental level of its morphological substratum. Function is, under normal conditions, an expression of structure. It would be impossible to find two developing individuals of the same species and of identical chronological ages, in which the level of development of corresponding organs or structures was identical. Even in monochorial twins, differences exist in corresponding parts. Some individuals are more advanced than others in certain respects, others more retarded than the average. Gesell and Thompson (1938) have recognized this normal variation in their study of the appearance of activities during the development of young children. In presenting their data, they give a "critical age" for each particular activity.

This is the first age at which the activity appears in at least 50 per cent of cases observed, for increasing items, or the last in which it is still in the fiftieth percentile, for disappearing activities. They also give the quartile values for the two preceding and for the two succeeding age levels. By this means, the percentage of cases at any age level may be shown, giving the variation found. At the present time, the number of human fetal cases observed is too small to permit use of this highly desirable method of presentation. It illustrates, however, the futility of attempting to split hairs in assigning an exact age to the appearance of any reflex.

The other consideration concerns only observations made on mammalian fetuses where the placental connections have been disturbed. When this occurs, the slow asphyxiation, which inevitably ensues, constitutes a modifying factor difficult to evaluate, but which must be considered. Though it is believed that this factor has been taken fully into consideration in the observations made in this study, further experience may prove that it was not given sufficient value and definitely affected the results secured.

Perhaps the widest separation in interpretation, between the work of Minkowski and that detailed here, concerns the activity of the thumb. Minkowski believes that the fetal thumb may be flexed, abducted and, sometimes, opposed. Within the age span covered by this study, no movement of the thumb that has been interpreted as true opposition has been observed. Indeed, no considerable movement of the thumb followed tactile stimulation at any age studied. Direct mechanical and, especially, faradic stimulation of the muscles of the thumb cause their contraction, with consequent definite movement. However, the fetal grasping reflex, before 25 weeks of menstrual age, is of the fingers only. Again, the difference between the two interpretations may be due to the difference in method of observation.

The grasping reflex is a component of prehension, as Halverson (1931) has demonstrated. Since the other components involve the exercise of capacities not present in the

fetus (vision, eye-hand coördinated movements, etc.), there can be no discussion of prenatal prehension. Grasping, however, has a fetal origin, and both a fetal and postnatal development.

Conclusions

1. Observations on the development of the grasping reflex in human fetuses prove the independence of its two phases, finger closure and gripping, as they develop sequentially.

2. Finger closure appears as a quick, but only partial, flexion of the digits, except the thumb, at about 11 weeks of menstrual age. Finger closure becomes progressively more complete, so that, by 16 weeks, a "fist" is generally formed on tactile palmar stimulation.

3. Gripping has first been observed about the 18th week. Though increasing in strength with age, it is still feeble at 25 weeks, the terminal age of this study.

4. Although it becomes feebly motile at about 12 weeks, the thumb seems to play no role in the fetal grasping reflex during the age span studied.

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NEUTRALITY *VERSUS* ALLIANCES

Belgium and the Revolution in International Politics

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ABSTRACT

"There is no nation better qualified to play its part than Belgium, who has suffered so much from centuries of international strife; Belgium, who by her own practice, has pointed the way to domestic prosperity for her people; Belgium who, from the first has always been ready to play her part in the creation of that new international order which is the objective of us all." (Sir Anthony Eden, Brussels, April 26, 1937.)

Always deeply involved in European international politics, and almost invariably drawn into the wars of Western Europe because of her geographical position, Belgium was made a perpetually neutral state by the five Great Powers at the London Conference after she obtained her independence in 1830. The neutrality established at that time was guaranteed by the Powers, but in 1914 it was violated by Germany, and after the war it was not again restored by the Peace Conference. Instead, Belgium joined France in a close military alliance, the first of a series of such alliances for security and the maintenance of the *status quo* established by the Paris Peace Treaties.

For a decade and a half the system of alliances continued, but with the resurgence of Germany on the one hand, and the signing of the Franco-Soviet Pact on the other, it involved commitments which many Belgians considered too dangerous. Serious divisions threatened to arise over the question even within Belgium itself. As a result the government decided on a radical change in its foreign relations. The new policy which it described as one of "independence and neutrality" was officially announced by the King on October 14, 1936. This article tells the story of Belgium's shift in policy and attempts to appraise its significance in relation to the larger revolution in the international politics of Europe in progress to-day.

ON October 14, 1936, the young king of the Belgians, Leopold III, startled the world by the abrupt announcement that "Belgium must pursue an exclusively Belgian policy, . . . a policy which aims solely at putting us outside the quarrels of our neighbors". Elaborating on the statement of his sovereign, Foreign Minister Paul-Henri Spaak added, "Belgium will be independent, neutral and loyal." The pact of

1920 with France, along with other military commitments, was accordingly abrogated.

This change in Belgian foreign policy, when taken in connection with the events that lie behind it, reflects a diplomatic revolution of major importance in European international relations. It is a tacit recognition of the radical changes that have occurred in the position of the European states since 1933. The Belgian declaration of 1936 is therefore, like the neutrality provisions of the thirties a century ago, an adjustment of the nation's foreign policy to the stern realities of the larger European situation.

In the present case, however, the Belgians themselves and not the Great Powers took the initiative. Neutrality was not imposed from without. It was adopted deliberately by the government after much careful consideration of the series of rapidly moving events in the international affairs of Europe during the preceding years. The advent of Hitler in 1933; the re-arming of Germany; the occupation and re-militarization of the Rhineland by Germany; the French alliance with Soviet Russia; the abandonment of the Locarno treaties; the decadence of the League of Nations, and the defeat of collective security revealed only too clearly the disruption of the international system established at the Paris Peace Conference. Belgian statesmen had to reckon with the return of Germany to the rank of a first-rate power, and the formation of the Rome-Berlin-Tokio axis in opposition to the powers supporting the *status quo* and the Peace Treaties.

The result of these startling changes in Europe's international affairs upon Belgian statesmen was the acceptance of the view, advanced by the critics of the alliances, that the system had serious disadvantages and risks which did not attach to neutrality. Much was made of the enviable position of Switzerland and of the Netherlands. As for collective security, that had not prevented the absorption of Manchukuo and Abyssinia by Japan and Italy. True, the fact that collective security had failed in Asia and Africa did not prove that it would also fail in the case of Belgium whose situation

was very different.[1] But Belgian statesmen, even though they shared this viewpoint as to the ultimate outcome in case of aggression in the west were equally sure that, as in August 1914, adequate military aid from her allies could not arrive in time to "parry the swift thrust of a modern army".

Added to the international objections to the alliance-system set up after the war was the hostility to it of many prominent Belgians for domestic reasons. Two political groups in particular, the Flemish and the Rexists, opposed it bitterly, refusing to vote for military budget increases until the nation was cleared of entangling alliances. On grounds of internal as well as international policy, therefore, the government had for some time entertained the thought of reverting to neutrality. Even King Albert I., who, more than any one else, had reasons to remember the tragic fate of neutrality in the world war, had seriously considered the possibility. A searching analysis of the obligations and implications of the alliances was undertaken by the government, and an elaborate dossier on the question was developed bringing together the arguments for and against the proposed change in policy. Summarizing the European conditions immediately associated with the adoption of Belgium's "independent and neutral policy" Foreign Minister Spaak attributed it to two set-backs in the international efforts at peace. "Je n'hesite pas à avouer qu'elle est fille de deux échecs: échec de Locarno, échec de la Société de Nations".[2]

The original neutralization of Belgium was first proposed by the London Conference in 1831 as a check to possible French designs of annexation following the revolutionary movements of 1830. Under the skillful guidance of Lord Palmerston, Prince Talleyrand was led to espouse the idea as his own.[3] France fell in line and the five powers formally

[1] Cf. *La Libre Belgique*, 17 Mars 1938; and Mr. Winston Churchill, in *The Times*, Oct. 30, 1937.

[2] Chambre des Representants, *Compte Rendu Analytique*, 16 Mars 1938.

[3] For a recent statement of this phase of the subject, see the author's "Belgian Neutrality: Its Origin and Interpretation," *American Historical Review*, Vol. XXXIX, No. 1, Oct. 1933.

guaranteed the independence and territorial integrity of Belgium as a perpetually neutral state.

Nevertheless, a neutral Belgium is a logical political response to the physical and economic geography of the country, for the very reason that its strategic location and topography have marked it as the natural arena for the wars of northwestern Europe. It lies at the crossroads of two of Europe's most important international highways, the route between England and the Rhine Valley on the one hand, and that connecting Paris with northern Europe on the other. Belgium is the center of the most populous, the most intensely industrialized, and the richest region of the Western World. In addition, the country has been for centuries a sort of cultural "no man's land" between two great peoples, the Latin and the Teuton, which have here found no natural barriers to define their political boundaries. As a consequence, the Belgian section of the Low Countries has been the scene of continual struggle. The unstable equilibrium of ethnic forces, coupled with the economic and dynastic ambitions of rival powers has led to frequent struggles for its conquest. Again and again powerful neighbors have sought to annex it; five foreign powers have possessed it within modern times.

On the other hand, other powers have been equally determined to prevent its absorption and, invoking the balance of power, have organized coalitions to check the aggressor and maintain Belgium as a sort of buffer state. But even these were generally opposed to its full development. When, for example, the Emperor Joseph II sought to stimulate the economic life of his Netherlands by freeing the Scheldt of the shackles imposed by the treaties, he was stopped by the jealousy of Holland and the opposition of England. For more than two centuries Antwerp, strategically the best located port in Europe, stagnated because of restrictions placed upon its commerce by jealous rivals. When the French annexed it during the Revolution, England declared war, and all through the revolutionary and Napoleonic period

the possession of Antwerp continued to be a major issue. "C'est à cause d'Anvers que je suis ici", said Napoleon at St. Helena, a logical conclusion to his earlier declaration that "Antwerp in my hands is a pistol aimed at the heart of England".

During the Revolutionary and Napoleonic Era, France conquered and annexed the entire country. Following the collapse of the Napoleonic Empire, the victorious powers, determined to prevent future aggression by France, not only set up a strong buffer state by uniting Belgium with Holland, but in a secret military protocol attached to the Quadruple Alliance, contracted that if the *casus foederis* should again be invoked, Dutch, English and Prussian troops were to occupy the fortresses.[1] The new state was to be adequately buttressed against revolutionary outbreaks and the annexationist tendencies of its powerful neighbor.[2] Nevertheless, for reasons well known to history, this union was destroyed by the revolt of the Belgians in the summer of 1830.

On August 25 they drove the Dutch from Brussels, proclaimed their independence, and proceeded to establish their own government. Consternation and alarm prevailed among the powers. The action of the Belgians not only disturbed the *status quo* but threatened to destroy the whole bulwark for security and peace which the powers had built up with so much care. Moreover, the July Revolution in Paris had made the situation doubly dangerous. Belgium in revolt might well invite the revolutionary elements in France to attempt to rectify the northeastern frontier, rid France of the humiliation of the border fortresses, and possibly annex the country. Under the circumstances, the appeal made to the powers by William, King of the United Netherlands, met with an im-

[1] The important instruments of the alliance were the Treaty of Chaumont of March 1, 1814, the Second Treaty of Paris of November 20, 1815, and the Protocol of November 15, 1818 signed at Aixla-Chapelle.

[2] The extent to which the same motives prevailed at the London Conference in 1830–32 is shown clearly by the fact that these agreements of 1814–1818 between the Four Powers were not abrogated but were incorporated in the secret Barrier Fortress Treaty of December 14, 1831, after the independence and neutrality of Belgium had been set up.

mediate response. A conference of the representatives of the powers was called in London to deal with the question.

The London Conference held its first session on November 4, 1830 in the British foreign office. The Duke of Wellington presided at its first meeting. Among the delegates, Prince Talleyrand was easily first in experience and reputation. He had served many masters, practically every government of France from 1789 to 1830. He was getting old—over 76—and he had had a strenuous life. Nevertheless, his colleagues felt that he needed watching, not only because of his reputation for ability and trickiness, but because they were suspicious of the newly established July Monarchy which he represented. So much impressed was Metternich with Talleyrand's cleverness and the dangers of the situation that he appointed a second delegate, Esterhazy, to assist Wessenberg, the Austrian Minister in London, to offset the great diplomatic skill of the prince. The Tzar was represented by his ambassador, Prince Lieven, but the remarkable letters of his wife, the Princess Lieven, to the Tzar indicate that she knew more of what really went on than her husband, the official representative. Heinrich von Bulow, a relatively young man, represented Prussia. He was extremely painstaking and Wellington, who respected his industry, once dubbed him a "wise fool" who was always discovering something remarkable in every trifling incident. His reports of the Conference proceedings, not yet published, are excellent. Aberdeen attended the first two sessions for England, and then gave way to Palmerston, the new foreign minister, a vigorous man of 46, "very well dressed, very good looking, with large black whiskers". He had been secretary of war from 1809 to 1828 in five different ministries, and was thoroughly schooled in the policies of Castlereagh. Soon to become the most English of the long line of English foreign ministers, he was, like the other delegates of the Four Powers, extremely distrustful of France.

The Conference met at intervals over a period of nearly two years from November 4, 1830 to October 1, 1832. The results of its arduous labors were embodied in the protocols of

some seventy sessions, innumerable notes, dispatches and memoranda, and in three important treaties.[1] The problems confronting the delegates were extremely complicated and difficult; according to Talleyrand "the most perplexing" of any with which he had been associated. However, after long and difficult labors, the Conference incorporated in the treaty the now famous Article VII establishing the new state. It stipulated that:

"Belgium within the limits specified in Articles I, II and IV shall form an independent and perpetually neutral state. It shall observe the same neutrality toward all other states".[2]

In the decades that followed, the neutral position of Belgium was frequently threatened, giving rise to international crises in 1840, 1869, 1870, 1875 and 1887. Belgium fell within the so-called "natural limits" of France—the Rhine, the Alps and the Pyrenees—so enthusiastically proclaimed during the Revolution. Thiers, and more particularly Napoleon III, looked on Belgium as the legitimate object of French expansion. In 1840, the former declared the treaties of 1814 and 1815 no longer operative. Napoleon III regarded the ultimate annexation of Belgium as a cardinal phase of his foreign policy. According to one of his biographers, he looked on the erection of the kingdom in 1830 as an artificial creation directed against the greatness of France and therefore without

[1] The official report of the proceedings of the London Conference was published by the governments of England, France, Belgium and the Netherlands, *e.g.*, *Papers relating to the Affairs of Belgium*, presented to both Houses of Parliament, 3 pts. (London, 1833); (also *Journals of the House of Commons*, 1833, appendix D, State Papers); *Collection des protocols des conférences tenues à Londres* (Paris, 1833); *Rapports due Ministre des Affaires étrangères . . . et autres documents publiés par l'ordre du Congrès national* (3 vols., Brussels, 1831 ff.); *Recueil de Pièces diplomatiques relatives aux affaires de la Hollande et de la Belgique, en 1830 et 1831* (3 vols. The Hague, 1831–1833). An extensive literature consisting of over 500 titles exists on the various phases of Belgian neutrality and independence. Aside from the memoirs and correspondence they fall in general into several groups: First, those that are essentially of a historical nature like the works by De Ridder, Guyot, Hampe, and Strupp; second, those devoted more particularly to the question of international law involved in neutrality agreements; and third, the many studies dealing with the military aspects of the Belgian situation including, of course, the problem of the border fortresses.

[2] Article VII of the treaty of 1831 and 1839. English text, *Common's Journal*, LXXXIII, 1068, 1094.

any claims to inviolability. After his success in securing Savoy and Nice, he made Belgium the special object of his attention. A project to gain control of the Belgian railway system was preceded by a definite proposal to Prussia for the annexation of Belgium by France as the price of her recognition of Prussian expansion in Germany. In August 1866 he instructed Count Benedetti, his Ambassador in Berlin, to propose a treaty to Bismarck, the fourth article of which reads:

> "On his part his Majesty the King of Prussia, in case his Majesty the Emperor of the French should be obliged by circumstances to cause his troops to enter Belgium, or to conquer it, will grant the coöperation of his arms to France, and will sustain her with all his forces of land and sea against every power which, in that eventuality, should declare war upon her".[1]

But events moved too quickly to enable the consummation of the plan even if Bismarck had entertained it seriously. After Sadowa the steps in the reconstruction of Germany followed in quick succession, and in 1870 the Franco-Prussian war broke out. As a first diplomatic offensive, Bismarck released the Benedetti treaty proposals. The effect on neutral opinion was conclusive. Troubled by the manifest disregard of the treaty guarantees to Belgium by two of the signatories, Gladstone induced both France and Prussia to make special agreements to respect Belgian neutrality. Although the action was regarded by some as an assumption that the guarantees of the London Conference were no longer valid, Gladstone did not look on it in that light.

With the development of the two great coalitions after 1878 and the rivalry between the Triple Alliance and the Triple Entente in the early years of the 20th century, Belgium was imperceptibly drawn into the orbit of the latter. Under the impending threat of a German invasion of France across Belgium in accordance with the von Schlieffen Plan, military conversations were carried on from 1906 to 1914 between the Chiefs-of-Staff of the western Entente Powers. How far these violated the spirit of neutrality is a matter of much

[1] *The Times*, July 25, 1870.

debate. In any case, the beginning of the World War promptly brought with it the execution of von Schlieffen's Plan and the fateful violation of Belgian neutrality by Germany.

The full import of the violation will never be known. The influence of moral values in history cannot be appraised accurately or quantitatively. In this instance, however, their effect was tremendous. The flagrant breach of an international pledge, though not without many precedents, shocked the moral sense of the general public in all countries and created a sort of ethical excuse for the advancement of more distinctly material and nationalistic interests. Referring to the treaty of 1831 in the course of his memorable speech before parliament on August 3, 1914, Sir Edward Grey said: "It was one of those treaties which are founded, not only on consideration for Belgium, which benefits under the Treaty, but in the interests of those who guarantee the neutrality of Belgium".[1] After the war Sir Austen Chamberlain expressed the idea in equally terse and even more realistic terms. "A friendly power in possession of the channel ports," he told the Commons, "is a British interest, treaty or no treaty." [2]

The system of post-war alliances developed rapidly after the defeat of the Central powers. At the Paris Peace Conference in 1919, the fears and passions of the war were entirely too strong to afford room for the consideration of Belgium as a neutral state. Neutrality was definitely discarded.[3] Article thirty-one of the Treaty of Versailles provided that,

> Germany was to consent to the abrogation of the treaties of 1839, by which Belgium was established as a neutral state, and to agree in advance to any convention the allies and associated powers might enter into to replace them.

The kind of convention alluded to in this article was doubtless the security pact agreed to between President Wilson, Mr.

[1] *Parliamentary Debates*, Fifth Series, LXV, 1819, Aug. 3, 1914.
[2] *Ibid.*, CL, 198–9, 1922.
[3] Cp. Ridder, A de, "L'Allemagne et la question belge en 1918," *Revue Catholique*, Avril, 1928.

Lloyd George and M. Clemenceau. Although signed by Mr. Wilson, the proposed treaty was later rejected by our Senate and, as a result, the Franco-British guarantee treaty did not become effective either. France then turned to a plan of bi-lateral arrangements which rapidly set the vogue for a long series of treaties, till there was developed the framework of a system of alliances in which practically every European country was in some way or other enmeshed.

The treaties all professed to be purely defensive and motivated solely in the interests of peace. But they all assumed that the preservation of the *status quo* and the security of the signatories were identical with the preservation of peace. They provided for concerted military action in case of aggression, the steps to be taken being set forth in special military conventions drawn up by the general staff of the signatories. The first of the treaties of this character was the Franco-Belgian pact, signed on Sept. 7, 1920. Contrary to Article 15 of the Covenant, the text was not submitted to the League Secretariat for publication. Instead, only an exchange of notes between the two states announcing the alliance was registered at Geneva. Similar conventions were made by France with Poland, and with Czechoslovakia in 1921 and 1924 respectively.

The Franco-Belgian treaty in particular reflected the sense of solidarity established between the two countries by the war and the memories of common suffering. France and Belgium became willing partners and for some years they acted in perfect unison in foreign matters where the interests of both were involved. England too was drawn into the understanding and a sort of post-war entente cordiale developed. On its military side, the entente involved the cooperation of Belgium and the use of her territory by the allies under the treaties and the Covenant of the League of Nations. As late as 1936, Mr. Stanley Baldwin, Prime Minister of Great Britain, went so far as to state publicly that "the defenses of England were no longer at the chalk cliffs of Dover, but on the Rhine". More than at any other time in her history,

the foreign policy of Belgium, her diplomacy, and even her military plans, were definitely integrated with those of her two powerful western neighbors.

But the policy of security as just noted did not stop with developing a system of alliances to maintain the *status quo* in the west only. Gradually, the system was extended to east central Europe also. Building on the work of the peace treaties in which the creation of Czechoslovakia, the dissolution of Austria-Hungary, and the strengthening of Rumania were supposed to offer insuperable barriers to the revival of German hegemony, France added a series of political and military alliances with the states in that region also. Bilateral treaties were made with Czechoslovakia, Poland, Rumania, and Yugoslavia. By these, she obligated herself directly, and her allies indirectly, to go to the assistance of her friends in eastern Europe in case one of them was attacked by another power.

The merits or demerits of the system do not concern us here. To the casual observer the system looked impregnable. With the advent of Hitler, however, the alliances involved problems not inherent in the system so long as Germany was disarmed. Fearful of the re-armed Reich, France extended her security pacts still further by the alliance with Russia in 1935 and the abortive Laval-Mussolini accord with Italy. This completed the system. It now seemed fully buttressed on all sides. Yet in the short space of three years it broke down.

Too many opposing elements had been brought into it. The smaller powers became restless and suspicious. True, the Franco-Russian pact brought the promise of additional support to Czechoslovakia from the Soviets, but the other members of the Little Entente disliked the Communist affiliation. Poland began to waver in her allegiance and came to an understanding with Germany. Even England found too obvious the parallelism with the pre-war alliance between the Third Republic and Tsarist Russia. Then came the failure of Barthou's policy to establish a united Franco-British-

Italian front in regard to Abyssinia. Mussolini's price was too high. In the meantime the revelation of the proposed attack on a fellow member of the League of Nations so aroused British public opinion that the whole Hoare-Laval-Mussolini accord with the Duce had to be scrapped. Instead of a joint dismemberment or division of Ethiopia into spheres of influence, Mussolini proceeded to its conquest by himself, the application of economic sanctions to stop him, ending in complete failure. Taken in conjunction with the seizure of Manchukuo by Japan, Germany's withdrawal from the League and the remilitarization of the Rhineland, it shook the shattered French system of alliances and put an end, for the time being at least, to the theory of collective security. The Rome-Berlin-Tokio axis developed to offset the Paris-Moscow-London entente.

From the standpoint of the Belgians, the situation had particularly serious and dangerous possibilities. Long before the autumn of 1936, Belgian statesmen realized that the international situation of Europe had radically changed from that which prevailed during the years from 1918 to 1933. The doctrine of *rebus sic stantibus* obtruded itself at every point in current European international relations. With a re-armed and powerful Germany on her border; a militant Italy challenging British power in the Mediterranean; and a France, weakened by internal troubles and compromised by the pact with the Soviets, the reasons for the alliance system, so far as it related to Belgium, had virtually disappeared. A revolution had taken place in international politics which called for a serious reconsideration of Belgium's foreign policy. To this was added further the strong hostility to the French alliance of certain political groups in Belgium itself. It appeared in many otherwise purely domestic questions, and it was in response to this that the Foreign Office in particular had for some time been giving considerable thought and study to the situation. The result was the somewhat abrupt but decisive announcement on October 14, 1936 by the King in Cabinet Council.

In his great speech to his Ministers on October 14, 1936, Leopold III, after referring to the recent changes in the European military situation, turned to the question of Belgium's international relations.

"Our military policy, like our foreign policy, which is necessarily determined by it, must be directed," he said, "not to preparing a war, more or less victorious, as the result of a coalition, but to keeping war away from our territory.

"The reoccupation of the Rhineland the breaking of the Locarno agreements in the letter and the spirit, placed us almost in the international position we occupied before the war. Our geographical situation compels us to maintain a military machine big enough to dissuade any one of our neighbors from using our territory for attacking another state. By fulfilling this mission Belgium contributes eminently to the peace of Western Europe, and creates for itself, *ipso facto*, a right to the respect and, if need be, the assistance of all states which are interested in that peace.

"On this basis, I believe, Belgian opinion is unanimous. On the other hand, our undertakings should not go beyond that. Any unilateral policy weakens our position abroad and, rightly or wrongly, arouses divisions at home. Even a purely defensive alliance would not lead to the desired end, for, however, prompt the assistance of an ally might be, he could not intervene until after the invader's attack—which would be a lightening attack—had been launched. In resisting that attack, we should be alone in any case. Unless Belgium herself has a system of defense capable of resisting it, she will find herself from the outset deeply invaded and immediately plundered. This stage past, friendly intervention might it is true, bring final victory, but the struggle would cover the country with ravages of which those of the Great War afford only a pale reflection.

"That is why, as the Minister of Foreign Affairs recently said, we must follow a policy exclusively and entirely Belgian. That policy should aim resolutely at placing us outside any disputes of our neighbors. It responds to our national ideal".[1]

A policy exclusively Belgian and entirely neutral was, however, entirely incompatible with the system of alliances into which Belgium had been drawn after the World War. The alliance with France, the close cooperation in military matters with England, and the indirect obligations of the

[1] For the English text here quoted see *The Times*, Oct. 16, 1936.

treaties made by France with other European powers had to be abandoned. Little wonder that so revolutionary a policy in international politics caused a crisis in diplomatic circles. Prime Minister Van Zeeland found it necessary to make a hurried visit to Paris and, later, to other capitals, while the French Foreign Minister, Delbos, and England's Foreign Secretary, Eden, each spent several days in Brussels in conference with the Premier, and Foreign Minister Paul Henri Spaak.

The policy so dramatically announced by the King was subsequently elaborated in greater detail both in the press and in the debates of the Senate and the Chamber of Deputies. Speaking before the Deputies in the great debate on the subject early in December, 1937, Premier Van Zeeland explained that the new policy was one of "independence, not isolation". Paradoxically, however, he later found it necessary in response to interpellations on the subject to say, "if we are not attacked, we remain neutral".[1] This came fairly close to isolation, and rather justified the somewhat ironical remark of a British diplomat that "The Belgians know they will be guaranteed, so they want to get out of guaranteeing". A year later, the Reporteur of the Committee on Foreign Affairs, M. Cauwelaert, told the Chamber (he spoke in Flemish):

"Cette indépendance nous impose une politique de neutralité à l'égard de ses mêmes puissances, . . . Elle nous impose aussi d'établir notre statut défensif de telle manière qu'il ne soit spécialement dirigé contre aucun de nos voisine, mais qu'il soit de nature à nous garder de toute agression, d'ou qu'elle vienne".[2]

Concerning the events that immediately led to the announcement of the new policy, Foreign Minister Spaak said:

"If Germany had not on March 7, 1936, occupied the left bank of the Rhine and if the conflict between Italy and the League of Nations had not ended in the defeat of the latter, those responsible

[1] Chambre des Representants, *Compte Rendu Analytique*, 2 Dec., 1937.
[2] *Ibid.*, 17 Mars, 1938.

for the nation's foreign policy would not have been under the necessity of finding a new international status for Belgium".[1]

To these should be added the following forceful re-capitulation of reasons given in the King's speech:

". . . the re-armament of Germany following the complete militarization of Italy and Russia; the transformation of methods of war under the influence of technical progress such as aviation and mechanization; the military reoccupation of the Rhineland; the weakening of the bases of international security by infraction of conventions, even those freely entered into; the virtual impossibility of adopting, in present circumstances, the provisions of the League of Nations Covenant and finally, internal dissensions in certain states which are liable to end in rivalry between the social and political systems of other states and produce a more desperate and devastating upheaval than those whose repercussions are still being felt".

In addition to the reasons for the new policy given by Leopold and his Minister of Foreign Affairs, there were several others. One already referred to above, the Franco-Soviet alliance, was hinted at by the king. It was very unpopular with many groups in Belgium as it was with all conservative interests throughout western Europe. Many Belgians also watched with considerable distrust the drift of domestic politics under Leon Blum and the Left Front in France. These fears and suspicions were naturally increased by the formation of the alliance with Russia. The Rexist journal, *Le Pays Réel*, continued to harp on the theme "Peace or civil war in France".

The re-militarization of the Rhineland by Germany; the civil war in Spain and its European ramifications; the labored efforts of the London Non-intervention Committee of twenty-seven nations to develop a policy of non-intervention there; the piratical sinkings by unidentified submarines in the Mediterranean, involving a direct threat to British and French lines of Empire communication; and finally, the Sino-Japanese war caused the "jitters" in the foreign offices of all the major European powers.

[1] *Ibid.*, 16 Mars, 1938. Cf. also *La Libre Belgique*, Mar. 17, 1938.

Another and more immediate reason for the announcement of the new policy, not appreciated, save by those directly in charge of the conduct of foreign affairs, was the serious threat of a European war in the summer and autumn of 1936. An account of the crisis is not necessary to this study, but it is a matter of interest that our own Secretary of State, Cordell Hull, referred to the danger as imminent and menacing, and indicated that our own neutrality policy was in no small degree developed in direct response to it.

In Brussels the situation caused unconcealed alarm and forced the announcement of the new policy. That it had been under consideration for some time, is evident from the broad hints thrown out by Foreign Minister Spaak in his speech before the Assembly of the League of Nations in Geneva in September. While the crisis was on, the government cancelled all military leaves-of-absence. Soldiers with the colors, who were due to be released on May 28 from military duty, were ordered to remain—a new army bill had been presented to the Chamber of Deputies on Feb. 11, 1936, increasing the term of military service of all classes. On March 30, a royal decree had appointed a mixed commission under a socialist deputy as Chairman to consider the re-enforcement of the national defenses. On April 16, the Defense Bill, which had encountered bitter opposition, was passed by the Deputies. It provided for strengthening of the defenses on the east from Arlan to the Dutch frontier. Following an unpleasant incident with Germany at Eupen a decree was issued providing for a civilian alarm system with sirens at each local alarm center. But the crisis passed, and with it the hysteria of fear.

The reaction in Belgium to the assertion of independence and neutrality in the nation's foreign relations was one of almost universal approval. "Le Roi a exprimé la volonte et les espoirs de tous les Belges. La presse approuve le discours royal", said *Le Soir*.[1] Press and platform expressed themselves freely and with much understanding. The people

[1] *Le Soir*, 17 Oct. 1936.

welcomed it not only because of the release from the obligations of the military alliance, but also because it removed a serious obstacle to the solution of certain domestic issues. These could now be considered on their own merits without reference to foreign engagements.[1]

In commenting on the report of the Committee on Foreign Affairs drawn up for parliament by M. Van Cauwelaert, Jules Gallis, the well-known political writer, voiced the public sentiment on the new policy fairly accurately.

"In the matter of neutrality, a policy to which we have reason to attach ourselves without hesitation, M. Van Cauwelaert maintains 'we can render the greatest service to the peace of western Europe by safeguarding our frontiers against all attempts at aggression'. That indicates precisely and fairly just where our duty lies. For it is natural that we should not rely blindly on the promises of protection of other powers. . . . We have the obligation to prevent, cost what it may, any power whatsoever from entering among us. . . ."

"Keeping war out of Belgium" gradually became an important aspect of the new policy in the popular mind. Even the mild criticisms of the change gradually subsided, and the favorable reaction became well nigh unanimous.

"The simple truth is, as it is also the most fortunate", said Spaak, the Foreign Minister, in introducing the great debate on the Budget last March, "that the head of the state at a critical moment was able to see what was for the greatest national good, and then translate it into superb language in accord with the sentiment of the great majority of the people. . . . I am convinced that it is in accord with the sentiments and the judgment of the great majority of our countrymen".[2]

The popular approval of the government's course among the Belgians themselves doubtless made them peculiarly sensitive to the reception of the new policy abroad. Foreign comments were given front page space throughout the entire discussion. Favorable opinions seemed especially welcome,

[1] Ydewalle, Charles D., "La fin de Locarno." *La Revue Catholique*, 13 Mars, 1936.

[2] Chambre des Representants—*Compte Rendu Analytique*, 16 Mars, 1938.

while surprise and, at times resentment, appeared at any unfavorable foreign criticism of the King's speech. Belgians resented the implications on the part of the foreign press that Belgium was faltering in loyalty to treaty commitments and to old friends. According to the King, "loyalty" was a special characteristic of the new policy. A military alliance was not necessary to establish that. To the disappointed proponents of the League of Nations and collective action in international affairs, Spaak replied, "Collective security and mutual assistance are excellent, but as yet they are fragmentary".[1] The tragic fate of Abyssinia in the preceding year was frequently mentioned to give weight to the argument. As the discussion progressed, the Belgian press repeatedly pointed out that the collective system had been killed in the Abyssinian crisis by Laval's evasions, and that Belgium was becoming tired of being a pawn in international diplomacy. Even the Walloons criticized the Franco-Soviet pact, and all, save the Leftist press, viewed the policies of the Popular Front government in France with deep concern. On the day following the King's speech, *La Nation Belge* carried the following in heavy type: "Le discours du Roi souligne pour l'opinion française, la nuisance du pacte franco-soviètique".[2]

A considerable part of the press also emphasized the German claim that the reasons for the renunciation of Locarno were to be found in the Franco-Soviet pact with which the majority of the Belgians were not in sympathy and which related to matters in which Belgium was only indirectly concerned. Writing from Brussels, the special correspondent of the *London Times* reported:

> "Recent external complications have made Belgium move away from the Quai-d'Orsay. Her policy now is neither pro-French nor pro-German nor pro-British, but Belgian. There is noticeable an increasing resentment of the French tendency to belittle *les petits Belges!* This also manifested itself in the elections of May 24, in which the new Rexist party gained 21 seats, and the Flemish nationalists increased their representation from 8 to 16.

[1] Address before the Socialist Federation.
[2] *La Nation Belge*, 15 Oct., 1936.

(Both were opposed to the French alliance.) Even the Walloons, shocked by the Franco-Soviet pact and displeased with the trend of internal politics of the Third Republic, became critical of France. The opponents of the defence program submitted in 1931 insisted that Belgium did not want to see the defenses on the east, and the Antwerp-Ghent line of fortifications against a possible invasion by Germany through Holland, merely an extension of the French Maginot System into Belgium".[1]

In France, England and the United States, the general effect of the King's speech was in many quarters quite disconcerting. The public was manifestly not prepared for so independent and so radical a departure from the accepted policies of the post-war years. Had not the war demonstrated the futility of Belgian neutrality in the face of *Machtpolitik* to which Europe seemed to be returning? Men recalled Bethmann-Hollweg's "Scrap of Paper" and the fact that neutrality had not protected Belgium in August, 1914 against the claim that "Necessity knows no law". It had not saved her from German occupation during the four years of the Great War despite the fact that the five principal powers of Europe had guaranteed the original neutrality in 1831 and 1839, and had tacitly endorsed it on different occasions later. Men remembered that the Paris Peace Conference at the request of Belgium itself, had not only turned its back on Belgian neutrality, but in the political section of the Treaty of Versailles had definitely renounced it, compelling Germany to subscribe to the renunciation. Despite the defeat of Germany, *Machtpolitik* had triumphed. The Treaty provided that:

"Germany, recognizing that the Treaties of April 19, 1839, which established the status of Belgium before the war, no longer conform to the requirements of the situation, consents to the abrogation of the said Treaties and undertakes immediately to recognize and to observe whatever conventions may be entered into by the Principal Allied and Associated Powers, or by any of them in concert with the Governments of Belgium and of the Netherlands, to replace the said treaties of 1839".[2]

[1] *The Times*, March 16, 1936.
[2] Article XXXI of the Treaty of Versailles.

Commenting on the attitude of the foreign press, *La Nation Belge* summarized the reaction in the title of its article as follows: "En France: incompréhension. En Angleterre: calme. En Allemagne et en Italie: accueil favorable". The lack of comprehension on the part of French public opinion and the failure of the French press to appreciate what it described as a strange drift in the conduct of Belgian foreign affairs was not surprising. France looked on the alliance as a sort of an official recognition of brotherhood-in-arms; of suffering and triumphs shared in common. She regarded the military convention as an integral part of her system and a basic feature of her foreign policy in central Europe. The renunciation of these by Belgium therefore affected the whole system of international policies of France most seriously. Moreover, unilateral action in matters of such importance was inconsistent with international practice. Some journals claimed that if the policy suggested in the king's speech were pursued to its logical conclusion, it would shake to its foundations the whole system of collective action set up in the Covenant of the League of Nations, a system France in particular had sponsored. True, Japan, then Italy and more recently Germany had defied it, but up to the present the friends of the League had remained loyal to the principle of the Covenant.

Fully appreciating this, the Belgian government did everything in its power to harmonize its action with its friendship for its former allies. Its Ambassador in Paris tried to soften the effect of the announcement of the new policy by assuring the Quai d'Orsay that the speech was merely an indication of the trend of Belgian policy—a change in emphasis. Belgium would, he said, remain faithful to her obligations. The King's speech, he pointed out, was partly dictated by the necessity of winning over certain parties of the opposition to the passage of the defense bill. At the same time, however, he drew attention to the fact that the Rexists and Flemish Nationalists were actively opposed to the Franco-Belgian alliance and that the Belgian people generally disliked the Franco-Soviet pact.

The situation was so critical that, as noted above, Prime Minister Van Zeeland himself made a hurried visit to Paris. On his return he reported to the Cabinet that he had confirmed his Ambassador's statement, but by further elucidations of Belgium's attitude, had won over the Quai d'Orsai to his government's point of view.

The new orientation of Belgian policy became the subject of an animated debate in the French parliament and press in the spring of 1937. The discussion brought out very clearly how much store France had placed on the alliance. Most of the points developed by the press were reiterated in the debate, though the tone was distinctly more moderate. France did not wish to antagonize Belgium or hold her to an engagement which had become irksome and difficult. Every nation had the right to make its own decision. Besides time and reflection made it perfectly clear to the French that in any case, whether as an ally or as a neutral, Belgium would always be a formidable obstacle to German aggression against France.

British reaction was on the whole much better tempered and sympathetic. Indeed, there were those who looked upon the action of Belgium as a very salutary move toward the emancipation also of England from commitments which might ultimately drag her into a war over the eastern front. The Conservatives, led by England's realist premier, Mr. Chamberlain, favored the Belgian policy from the start. Like the Belgians, they didn't like the French pact with the Soviets, nor the alliances with the small states of eastern Europe. The system involved both Belgium and Britain in obligations, which neither was ready to continue longer as a policy. It savored too much of the pre-war conditions.

How far the British government was consulted by the Belgians before Leopold announced the new policy, is not known. That it was better informed than the French, and from the start much more cooperative in seeking a new *modus vivendi*, is clear. With this in mind, the British had renewed their assurances of military assistance to France and

Belgium following the reoccupation of the Rhineland by Germany, and joined with France in March, 1937, to seek an acceptable solution of the Belgian question.

In the negotiations that followed it soon appeared that Belgium was adamant in her insistence upon complete independence in the conduct of her foreign affairs and that the military alliance was abrogated. On the other hand, the obligations under the Covenant of the League of Nations were debatable because Articles XVI and XII had never been clearly defined. At first France refused to make any concessions in this respect. While both powers were prepared to release Belgium from any direct obligations to themselves they claimed that Belgium, as a member of the League, was bound to cooperate with other powers against an aggressor state, and if necessary recognize the claims for the right of passage of French and British troops.

The result of the negotiations was the draft of the British-French joint note of April 24, 1937, in which France and Britain released Belgium of all obligations under the Locarno Treaty and the arrangements of London of March 19, 1936.[1] The considerations on which the two powers agreed to release Belgium were set forth in the third section of the Note as follows:

"The said governments have taken note of the views which the Belgian Government has itself expressed concerning the interests of Belgium, and more particularly:

"(1) the determination expressed publicly and on more than one occasion by the Belgian Government: (1) to defend the frontiers of Belgium with all its forces against any aggression or invasion, and to prevent Belgian territory from being used for purposes of aggression against another State as a passage or as a base of operations by land, by sea, or in the air; (2) to organize the defense of Belgium in an efficient manner for this purpose.

"(2) the renewed assurances of the fidelity of Belgium to the Covenant of the League of Nations and to the obligations which it involves for Members of the League".

[1] *Command Papers*, No. 5437.

These conditions were further emphasized in a statement by Foreign Secretary Eden before the House of Commons on April 28.

> "Hon. Members will see", he said, "that by the terms of this document the two Governments declare that they consider Belgium released from all obligations toward them resulting from the Treaty of Locarno and the arrangements of 19th March, 1936, and that they have taken note of certain views expressed by the Belgian Government relating to their fidelity to the Covenant of the League of Nations and their determination efficiently to organize the defence of Belgium against any aggression or invasion".

Mr. Eden also pointed out that the release of Belgium, according to the terms of the note, followed an exchange of views between the signatories of the Treaty of Locarno looking toward "a new western European security pact"; that Belgium did not wish to give guarantees to other states; that the other five powers were prepared to agree that under the new treaty it would not be required of her; and that the note in no way effected the obligations of any other country.

By some authoritative sources, this Franco-British declaration was regarded as one of the most important diplomatic announcements of policy since the War. While it does not mean a re-alignment in the relations of European powers, it revealed a real shift in the importance of their respective roles. The unquestioned hegemony of Europe held by France during the decade after 1918 disappeared altogether. And more important still, the principle of collective action through the League of Nations in the conduct of important international affairs, was sacrificed.

Throughout the discussions of the new turn in Belgian foreign policy, Germany had noted every sign of the change toward neutrality with undisguised approval. The press of the Reich had made it plain that it saw in the Franco-Belgian alliance a policy hostile to Germany, and that a shift to neutrality would be greatly welcomed. It greeted with much satisfaction Foreign Minister Spaak's speech declaring that

Belgium would support all efforts for world peace, but that while that work was going on "the tranquility and security to which Belgium has a right should be assured by her neighbors". The remarks were characterized as "sober and rational" and in full accord with "the great proposal of the Fuehrer". Consequently, Chancellor Hitler, in his address before the Reichstag on January 30, 1937, the anniversary of his advent to power, declared "the German government has assured Belgium and the Netherlands that it is ready to acknowledge and guarantee these states at all times as inviolable and neutral territory".

This was followed on October 14, the anniversary of Leopold's speech, by a declaration by the Reich in the form of an official note handed by von Neurath to the Belgian Minister in Berlin. According to it and the terms of a definitive agreement between Belgium and the Reich, Germany too, formally guaranteed Belgian neutrality and the integrity of her territory in any future war, unless Belgium should cooperate in a war-like operation against Germany.

"The German Government declares the neutrality and integrity of Belgium is of common interest to Western powers. It confirms the determination under no circumstances to impair such inviolability and integrity and at all times to respect Belgian territory except, of course, in case of an armed conflict in which Germany were involved, Belgium should participate in military action against Germany.

"The German Government is ready, just as the British and French Governments, to grant assistance to Belgium in case she should become the object of aggression or invasion".[1]

Both the Belgian and the German press expressed "profound satisfaction" over the settlement, the latter hailing it as a concrete evidence of Hitler's peaceful intentions—"a new expression of amity and non-aggression". Coming so soon after President Roosevelt's Chicago Address, German official commentators spoke of it as a realistic answer to the implied indictment of Germany by our President, Hitler himself

[1] *The New York Times*, Oct. 14, 1937.

declaring that international anarchy and lawlessness were not to be laid at the door of Germany.

The proviso that Belgium maintain a vigorous military program and provide for adequate national defense was continually stressed in the negotiations both at home and abroad. The conviction that the failure to do so before the world war had been responsible for the tragic collapse in 1914 had grown steadily till it became almost a fixed idea among statesmen and military leaders. From the very beginning of the public discussion it was evident that the new policy was closely integrated with the Defense Bill. Indeed, in the king's speech the military program holds first place. Collective security and guarantees are all right, but not as a first line of defense against an aggressor. The machinery for its enforcement and operation are too slow, as the king expressed it, "to parry the quick thrust of a modern army". The lessons of 1914 were not forgotten and were again brought out too clearly in the investigations to admit of any illusions about timely assistance in case of attack from abroad.

Speaking of the question on the occasion of the presentation of the Budget on Foreign Affairs on March 16, 1938, the Foreign Minister said:

> "La politique d'indépendance n'est pas une politique d'eufonie et de facilité. Elle repose tout entière sur une Belgique forte. Jamais je ne suis venu devant le Parlement avec l'intention de créer des illusions. . . .
>
> "Au contraire, j'ai insisté bien des fois sur la necessité d'une armée forte, et je me souviens d'avoir indiqué que c'est là ce qui différencie essentièllement le climat de neutralité d'avant 1914 d'avec le climat bien plus fier de l'indépendance".[1]

But even this and the vigorous armament program of the Defense Bill did not satisfy some. In the debates of both the Senate and the Deputies the question of the need of foreign aid was raised repeatedly. The proponents insisted on definite assurances on the matter. Thus, during the

[1] Chambre des Representants, *Compte Rendu Analytique*, 16 Mars, 1938. Cp. Also *Le Peuple*, 17 Mars, 1938.

concluding hours of the debate on the Budget for Foreign Affairs on March 7, the Catholic Deputy, Poncelet, after vouchsafing the support of his party to the Foreign Minister's program, asked that Spaak's policy give particular attention to the question of assistance from neighboring powers.

"For despite all solemn declarations of independence, was Belgium", he asked, "really independent? Was it not tributary to its neighbors in case of aggression of which it might be the object? What did we have in 1914? Nothing! French aid? It came only later, too late. Could Belgium withstand aggression by itself? It is a situation we do not want longer to contemplate. We wish to know from what quarter aid will come in case of need. . . . Nous devons savoir si, en dépit de notre politique d'indépendance et sans en rien abandonner, d'quel part, nous serons secourus par nos voisins et par lesquelles".[1]

As noted above, the most difficult problem involved in Belgium's new policy was its adjustment to the League of Nations. How to reconcile Belgium's new policy with her obligations under the League of Nations Covenant, particularly Article XVI, became the subject of protracted negotiations.

During the heat of the discussions a popular American commentator declared, "As long as Belgium remains in the League of Nations, it cannot expect to enjoy the advantages of neutrality".[2] This uncompromising statement represents the strict constructionist viewpoint. Theoretically, it was correct, but in practice it needed qualification. Certainly Belgium has remained in the League and is assured the advantages of neutrality by the Powers. But the Franco-British note also stressed "the renewed assurance of Belgium to the Covenant of the League of Nations and to the obligations which it involves for members of the League". This troubled the Belgian government a great deal. Foreign Minister Spaak addressed a note to M. Avenol, the Secretary General of the League, asking for a clarification of the obligations of League members under Article XVI, by the Assembly. He

[1] *La Libre Belgique*, 24 Mars, 1938.
[2] *Foreign Policy Bulletin*, April 30, 1937.

also expressed his government's desire for the elimination of the threats of war in Article XI. The replies of Geneva should throw important light on the unhappy process of the gradual breakdown of collective security.

In the meantime, the government defended its new policy with much spirit. But it was not always easy. Questions addressed to Foreign Minister Spaak during the debates on the subject at times embarrassed him considerably. His opponents wanted to know how he harmonized his protestations of loyalty to the League in his address before the Geneva Assembly and the new policy, the logical result of which would weaken allegiance to the Covenant. Irritated and at bay, Spaak returned to the defense of his position repeatedly. On March 16, 1938, in a notable address before the Chamber, he defined his policy against, what he called, "the background of actualities".

> "Since certain newspapers have accused me of saying one thing at Geneva and another at Brussels, will you allow me to quote myself? . . . to read certain extracts from the short address I made at that time before the one hundredth session of the Council of the League of Nations?".

He would, he said, state clearly his ideas on the League and the consequences which the application of the principles of the Covenant would bring in its train.[1] He acknowledged that at Geneva he had clearly proclaimed Belgium's fidelity to the League of Nations. He also admitted that his act of faith had lacked warmth and enthusiasm. Indeed, he confessed to a sort of shame at his lack of confidence in the League, a feeling shared by a great many persons, in the face of the developments in China. This was not a time, however, he went on to say "to condemn the principles of the League of Nations, but merely to draw attention to the fact that some of its principles were not applicable". "It is a condition which I deplore and regret and which I believe is only temporary, for some day men and people will learn that it is only by adhering to the right and by the organization of justice internationally

[1] Chambre des Representants—*Compte Rendu Analytique*, 16 Mars, 1938.

that peace for all can be assured. However, in awaiting that day and the new circumstances, I am forced to take account of realities. . . . The annexation of Austria by Germany is an accomplished fact. For divers reasons, neither Italy nor France nor England were able, or wished, to oppose it. The Belgian government which received notice of the event yesterday through the German and Austrian Ministers could not do otherwise than make note of the communication".[1]

Belgium cannot ignore the lessons of history. Therefore, Belgium has insisted on her own interpretation of the Covenant. Addressing the Chambre of Deputies on April 29, he said further:

> "Belgium by organizing her national defense is giving Europe all that Europe could legitimately ask of her. . . . Even the right of transit of troops to cross her territory in fulfillment of the obligations of Art. XVI of the Covenant must be made", he declared, "conditional on Belgium's own decision. . . . Under no circumstances should the right of transit be imposed upon Belgium without her consent and such consent was conceivable only if it was a question of carrying out common action".[2]

Rumors of like action by other states, notably by Rumania and Czechoslovakia, caused grave concern in European international circles interested in the League and collective security. Following the Belgian lead, President Motta of Switzerland declared that in the future his country must be absolutely neutral. Switzerland would remain in the League of Nations, but, he added, "Geneva cannot at any price be a seat of coalition". When Switzerland joined the League of Nations in 1920, she subscribed with considerable hesitation to the terms of the Covenant in regard to economic and financial sanctions, military sanctions having been definitely waived by the Council. After 1933, however, and the experiment with sanctions against Italy, the withdrawal from the League of Japan, Germany and Italy, and the abandonment of all thought of disarmament, the Swiss became very restless

[1] *La Libre Belgique*, 17 Mars, 1938.
[2] *The Times*, April 30, 1937, and the *New York Times* of same date.

and a strong movement developed for a popular referendum rescinding even the limited commitments concerning economic sanctions. Confronted with this the Federal Council, on April 29, 1938 requested the League to grant the release and on May 14 following, the Council of the League in a formal resolution granted the request. The parallelism with the case of Belgium is obvious. In the meantime, there came the announcement of a move for neutrality by the Scandinavian powers.

From the practical standpoint, Belgium gains a great deal and loses nothing. The new policy removes a major pretext for attack on the part of Germany and, while it sacrifices the French pact of mutual support, Belgium knows very well that she can always count on the help of France and Britain in time of need. It is to their self-interest almost as much as it is to that of Belgium, that Belgium be defended against the aggression of a strong European power. Under the new status, moreover, she is herself perfectly free to act as her interests dictate whenever a crisis arises. Nor should the bearing of the new policy upon the domestic situation be overlooked. The abrogation of the military alliances on the part of the Belgian government eliminated a serious source of internal dissension. Until that was done, the Defense Bill was blocked.

Furthermore, Belgian statesmen, better than those of any other country, understand the values attached to neutrality. They know that, notwithstanding its fate in 1914, the world's condemnation of German action has made neutrality a supreme issue in international relations and international morality. Despite its temporary abandonment at the close of the war, neutrality—and Belgian neutrality in particular—commands a unique respect among statesmen and peoples. The extensive literature on Belgian neutrality and its violation that has arisen, lends it a sort of halo, which is reflected in nearly all the textbooks on history and government of post-war years. Important moral values have become associated with the concept of Belgian neutrality and Belgian statesmen

wisely recognize the significance of its recapture. But the new policy represents a much more independent and more nationalistic attitude than was the case in the neutrality of the nineteenth century. Indeed as the discussion has progressed these aspects of the question have been more and more emphasized. Belgium will be "independent, neutral and loyal", said Foreign Minister Spaak, but "to pursue a policy of independence does not mean adopting a policy of isolation. . . . We have never failed nor recoiled when our responsibilities have been involved". It is a policy of independence and neutrality guaranteeing freedom of action in times of crises, buttressed by national preparedness and military strength. To students familiar with the prolonged and arduous labors throughout the ninety odd sessions of the London Conference a century ago, the action of the Belgian government in emancipating itself from the post-war alliances by a bold announcement of its sovereign power was a master stroke of diplomacy, one of rare courage and great statesmanship.

HARRISON, BLAINE, AND AMERICAN FOREIGN POLICY, 1889–1893

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(Communicated by William E. Lingelbach)

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THE "Empire Days" of the United States did not dawn with McKinley's administration, as some writers have indicated, but rather with Harrison's. This drift toward imperialism was indicated in diplomatic notes, congressional debates, newspapers, and lectures. In 1889, the year in which Harrison entered the White House, Edward J. Phelps delivered the annual Phi Beta Kappa address at Harvard on the subject, "International Relations"; he declared that the halcyon days of isolationism were doomed forever.

Surveying the record of the Harrison administration after its close, anti-administration editors called attention to its "policies of entangling alliances and intrigues," and "its series of departures of the gravest nature from the old and fixed traditions of the Government." "Are we ready," asked *Harper's Weekly*, on March 18, 1893, "to alter the whole character of the government, with its beneficent traditional policies, to impose upon the people the burdens entailed by the building up and maintaining of immense armaments, and to expose this republic to all the political and economic consequences which such a policy would bring in its train . . . ?" These editors welcomed the second coming of Cleveland to Washington, for he was to meet in combat and put to rout the forces of expansion. The rout, however, was only temporary; for Harrison's foreign policy, and not Cleveland's, gave expression to the deeper currents of American life during the period from 1880 to 1920.

Among its departures from old and fixed traditions, was its "big navy" program, carried out by Benjamin F. Tracy, one of the ablest of all American secretaries of navy. He worked in close personal accord with President Harrison and was one of the two or three most trusted of Harrison's counsellors. Under Tracy's direction, the process of replacing the old wooden ships with modern steel armored ships went on at a rapid pace. He saw the history-making battleship *Maine* launched in 1890, and in the next year, the great armored cruiser *New York*, with a speed of 20 knots and the ability to sail 13,000 miles without recoaling. The famous first class battleship *Oregon* followed in 1893, and, on November 5, 1892, the *Olympia*, flagship of Admiral Dewey at Manila Bay, was launched. These were the ships which, before the century closed, were to enable American imperialism to bear fruit. During the years from 1889 to 1893, cruisers flying the American flag were seen hurrying hither and thither in all quarters of the globe.

Before Tracy took office, Cleveland's Secretary of Navy had ordered Captain Alfred T. Mahan to sea. When Tracy's own Chief of Navigation took the view that a captain in the navy should sail ships and not write books, Tracy, with Harrison's full approval, over-ruled him and provided for Mahan the leisure which enabled him to produce his epoch-making books on the influence of sea power on history. In four years, Harrison's Secretary of Navy had brought results, world-wide in their influence, and had elicited a spontaneous outburst of national pride in both Democratic and Republican newspapers.

In an interview in the New York *World* on Nov. 26, 1891, Tracy declared: "the sea will be the future seat of empire. And we shall rule it as certainly as the sun doth rise! . . . To a pre-eminent rank among nations, colonies are of the greatest help." Tracy watched with an eagle eye all opportunities to secure new colonies for naval bases. In this he had the President's support. The latter wrote to Blaine on October 1, 1891: "You know I am not much of an annexationist; though

I do feel that in some directions, as to naval stations and points of influence, we must look forward to a departure from the too conservative opinions which have been held heretofore." Among the acquisitions which Harrison, Tracy, and Blaine sought to secure were the Danish West Indies, a lease on Samana Bay in San Domingo, a concession of the Mole St. Nicholas in Haiti, a naval base at Chimbote, Peru, all of Canada—including sole jurisdiction over Bering Sea and sole property rights in its fur seal—and a naval base in Samoa. Located thousands of miles outside of the natural sphere of the political influence of the United States, Samoa was to serve as a beacon to guide America to Guam and Manila. While his chiefs in Washington were thus planning acquisitions in the western hemisphere, Whitelaw Reid, Minister in Paris, was wistfully entertaining from Portugal offers of commercial entrepots and naval bases on the Azores, at Lisbon, in Angola, and even in Mozambique on the distant Indian Ocean.

It was with Chile, however, that the most strained diplomatic relations during Harrison's administration developed; war seemed almost inevitable at the close of 1891. Chilean animosity towards the United States was of long standing and grew out of unsettled claims of American citizens, the rivalry of English economic interests, and the unfriendly attitude of the United States during Chile's War of the Pacific with Peru and Bolivia. After the revolution broke out in Chile early in 1891, relations were still further strained when the United Stated seized the *Itata*, a Chilean ship carrying munitions from California to the revolutionists, and when asylum in the American legation in Santiago was granted to political refugees hunted by the revolutionists. Many diplomatic notes were exchanged on the latter subject; because of Blaine's illness, most of the American notes were written by Harrison with the aid of Tracy and the Assistant Secretaries, Wharton, Adee, and John Bassett Moore—though such notes went out over Wharton's signature, and not Harrison's.

On the sixteenth of October, 1891, when a settlement of the refugee question had been almost effected, an incident

occurred which threatened serious trouble; 117 members of the crew of the United States warship *Baltimore*, commanded by Captain Schley, had been granted shore leave in Valparaiso, Chile; towards evening, an infuriated Chilean mob murdered two and seriously wounded sixteen of the sailors. In answer to the Chilean accusation that the American sailors had been drinking in saloons, Commander Evans later observed that Captain Schley's "men were probably drunk on shore, properly drunk; they went ashore, many of them, for the purpose of getting drunk, which they did on Chilean rum paid for with good United States money. When in this condition they were more entitled to protection than if they had been sober."

When the news of this incident reached Washington, Blaine was back at his post. There was disagreement as to the responsibility for the attack and also as to its gravity. Blaine, with his Pan-American and reciprocity policies at stake, was inclined to treat the incident as accidental and of minor importance. Harrison, who as a soldier had worn the military uniform of the United States, regarded the attack as of gravest importance. Rival newspapers charged Blaine with letting his unfriendly feeling towards Harrison dominate his views, and accused Harrison of generating war enthusiasm in order to get reelected. When Blaine presented his views in Cabinet meeting, the President leaned forward, and, with an emphatic gesture, declared: "Mr. Secretary, that insult was to the uniform of the United States sailors."

It was under such circumstances that the President drafted the instructions of October 23, 1891, to the American Minister, Patrick Egan, in Santiago. These gave Chile an opportunity to present its side of the incident: but, if the facts were as reported, then, concluded the note, the American government "can not doubt that the government of Chile will offer prompt and full reparation." When Egan presented this note, the Chilean Foreign Minister replied that the United States was formulating demands and advancing threats which, "without being cast back with acrimony, are not acceptable," and

added, that a Chilean court would properly investigate the matter. The Anglo-Saxon sailors felt that their case was not being tried fairly in the strange Latin courts.

While the case was pending, Harrison had an opportunity to explain his position, when, on November 14, the new Chilean Minister, Montt, came to present his credentials. As usual in such cases, the Department of State had prepared a draft of the President's speech. It expressed Blaine's spirit and policy, and may have been prepared by him. After Harrison read it, he wrote on it in clearcut letters "Rejected," and then made a careful revision which he read to Montt.

A few weeks later, on December 12, there appeared in American newspapers a "diplomatic atrocity" in the form of a letter from the Chilean Foreign Secretary, Matta, to the Chilean Minister, Montt, in Washington. It had been read to the Chilean Congress before it was sent. In insulting words it charged that the President of the United States and his Secretary of the Navy had made official statements which were "erroneous or deliberately incorrect"; that there was "no exactness nor sincerity in what is said in Washington" with respect to the seamen of the *Baltimore*. It closed with asserting "the right, the dignity and the final success of Chile, notwithstanding the intrigues which proceed from so low a source, and the threats which come from so high a source." When Commander "Fighting Bob" Evans of the *Yorktown*, then anchored at Valparaiso, read this letter, he entered in his journal on December 12: "I don't see how Mr. Harrison can help sending a fleet down here to teach these people manners. . . . I certainly would like to hear what Mr. Blaine has to say in reply." Four days later, he wrote: "The papers here grow more and more insolent and I don't see how Mr. Harrison can avoid sending an ultimatum at a very early day."

American sailors and boats were now being stoned by Chileans. The Chilean naval commander held manœuvers in the harbor during which torpedo boats apparently used the *Yorktown* for target practice, running by within six feet. "I then saw the officer in charge of the drills," wrote Evans, "and

told him that he certainly had great confidence in the steering gear of his torpedo boats; that if anything should jam so that one of them struck me I would blow her bottom out. He replied that the water in the harbor belonged to his Government, and he proposed to use it for the purpose of drilling his boats. I answered that I was fully aware of the ownership he had stated, but that the Yorktown and the paint on her belonged to the United States, and that neither must be defaced by his torpedo boats. After this incident they did not run at us so much, though the newspapers encouraged them to do so." Evans spent sleepless nights in December. On the 28th he wrote in his journal: "the crew of the Yorktown will sleep at the loaded guns to-night, and every night until I get some better news."

Two weeks later, the Chileans finally permitted the refugees to leave the legation and to be carried to safety by the *Yorktown*. Evans now breathed a sigh of relief, but prematurely. What followed can best be told in his own words: "During the early part of the first night out, while running at very high speed, a suspicious noise was heard in the high-pressure cylinder of one of the engines. When the cylinder head was taken off I found two hardened steel wedges, which had been placed in the valve chest, no doubt, when we first arrived at Valparaiso by some evilly disposed person, with the intention of sending us all to the bottom. . . . It was simply a miracle that I escaped a serious disaster, for if the cylinder head had been knocked out with the pressure of steam I was carrying, the side of the ship would have gone with it. As I stood watching the group of machinists and firemen working about the engines I could see the blood come into their faces until the whites of their eyes were bloodshot. I knew pretty well what they were thinking. . . ." Only excellent seamanship and sheer good fortune had averted a tragedy which might have been as disastrous as the blowing up of the *Maine* in 1898.

Only two years before, Blaine had presided over his grand Pan-American Conference, which had advocated settling all

disputes between its members by mediation or arbitration—not by war. Chileans proposed such a settlement now, but it was to prove an irridescent dream. Concerning this alternative, Evans wrote in his journal on December 31: "When the United States is willing to submit the question of the murder of her sailors in uniform to arbitration, I must look for other employment—the Navy won't any longer suit me. They can arbitrate 'till the cows come home' about the people in the legation at Santiago, but if they ever hint at such a thing about the Baltimore's men, then I think the voice of the American people will be heard in no uncertain tones."

Meanwhile, in Washington, Harrison's private secretary, Halford, was writing in his diary on December 18, 1891, that the President feared there would be war with Chile. On January 1, 1892, he wrote: "The President stated that all the members of the Cabinet are for War." Evidently Blaine was vacillating. Elkins said to Halford on the 19th: "Blaine changes his mind every five minutes." Sir Cecil Spring Rice of the British legation wrote that Tracy wished for war to see his new ships fight, and to get votes for more, and that the naval officers were encouraged in their natural desire for distinction.

War preparations went on at a feverish pace in both countries during the winter. At Valparaiso, heavy Krupp guns were arriving from Germany, and Chile cabled for a Prussian major to supervise their mounting. Chileans were "working like beavers" to get their ships ready, while Tracy's navy yards were working day and night, including Sundays. At the request of Harrison, the Navy Department prepared an exact list of all warships showing the guns carried, and the number of days each would require to get to Valparaiso. Orders were also placed with Steyes in Austria for 3,000,000 cartridges. Blount, a leading Democratic member of the House Committee on Foreign Affairs, called to assure the President that the Democrats would not embarrass him and that whenever he wanted to come to Congress with the Chilean question, it would be treated outside of partisan politics.

Blaine's department was especially active. Peru, an old enemy of Chile, seeing a chance for revenge, was friendly to the United States, and was seriously considering ceding to it a permanent naval base at Chimbote. Both Harrison and Blaine desired such a base. Brazil's new revolutionary and republican government had just sent an Admiral with a fleet to Washington to present Harrison with a medal in token of appreciation of the early and friendly recognition accorded by the United States; hence Brazil would be expected to follow a policy of benevolent neutrality. The American Minister to Brazil was instructed to ascertain secretly the quantity and kind of coal, and the speed and capacity of steamers available in Brazil. Argentina, like Peru, had a boundary quarrel with Chile and was already on the verge of war. It had ordered its navy to be in readiness, and informed the United States that it could easily furnish the United States army with beef cattle and other supplies by a short route over the Andes.

The war fever was rising rapidly. On January 6, 1892, John Hay wrote Henry Adams that Brooks was profoundly disgusted with Harrison for not declaring war and that Theodore Roosevelt was going about hissing through his clenched teeth that the United States was dishonest. On the 18th, James Findlay Harrison, cousin of the President, wrote him: "It looks to us outsiders that there may be war with Chili, and in that event I hope you will not class military appointments for your kinfolk, as you do civil. The right of a Harrison to military service cannot be abridged and I think from education and experience that I am able to render some service. . . ." To this the President replied: "I think you make a good point on me. Certainly I ought not to be unwilling to sacrifice my relations—as Mark Twain would say—if there is war with Chili! I have always been hopeful that a sense of justice would lead the Government of Chili to do that which would avoid collision. I am, as you know, extremely full of urgent business and anxious thought and have only time to acknowledge your letter and to send you my affectionate greetings."

On January 19, Sir Cecil Spring Rice wrote: "We are on the verge of a war, here which is owing to inconceivable stupidity of our (that is, the American) side and trickery on the other . . . [Blaine] has prevented war with Chile so far, and may do so still; but the President and the navy are bent on it." Events were widening the breach between Harrison and Blaine, and self-seeking politicians in both parties were trying through the newspapers to bring about an open break. Blaine was ill early in January and lost or disregarded a request for a conference with Harrison.

On January 2, 1892, Blaine forwarded some Chilean despatches which he regarded as conclusive and, in effect, an apology. This Harrison denied in a letter to Blaine on the 4th. "It still seems to me," he wrote, "to have been an assault upon our sailors as such, and to have grown out of the unreasonable irritation and animosity which the Junta had promoted among the Chilean people . . . that it began by a Chilean sailor spitting in the face of one of our men, which was followed by a knockdown."

Harrison now asked Blaine to cable Egan for the complete records of the trial. These he studied carefully, together with all the diplomatic correspondence. On January 19, a grave Cabinet meeting was held. The President had prepared for discussion a two page memorandum on the crisis which suggested the form that the next note to Chile—virtually an ultimatum—should take. Before the Cabinet came to it, however, Blaine became ill and withdrew, and Harrison did not then present it. The memorandum called for apology and reparation for the attack on the *Baltimore* sailors, the withdrawal of the offensive Matta letter and a suitable apology for it; if these were refused, diplomatic relations were to be broken off and all correspondence submitted to Congress.

The very next day Chilean resentment reached its peak in a despatch which declared that Egan was no longer *persona grata* and asked for his recall. So breathlessly acute was the situation now, that when this note arrived at the Department of State the exact time of its arrival, 1:23 P.M., was marked

on it, and within half an hour Adee had a deciphered copy on the way to the White House. In reply, Harrison dictated a note which, fortunately, was not sent. It would have resulted in handing passports to the Chilean Minister in Washington, and in the recall of Egan.

On January 21, another draft, prepared at the Department of State, probably by Blaine, was sent to the President for approval. It was an indecisive note declining to recall Egan. On the margin of this draft, still preserved among his *Papers*, Harrison wrote: "This prepared by State Department—but my note was substituted." Among his *Papers* is also preserved the clear copy of his note in his own hand with but few erasures. In sharp contrast to the Department's draft, Harrison's note brushed aside the question of Egan's status, and in clear terse statements presented the substance of his Cabinet memorandum of the nineteenth as a virtual ultimatum. At the top of Harrison's draft—a document significant in the history of the United States—appears this note in his own hand: "After a talk with Mr. Blaine we agreed to put this in shape of a note to Egan and I made on the face of typed copy the necessary alterations. Mr. Blaine said it was just right, took high ground, and he approved every word of it."

The ultimatum, in Harrison's words, was sent on January 21 as instructions to Egan signed by Blaine, to whom historians have ascribed it. Ultimatums are usually answered within twenty-four hours. When no reply had come after four days, Harrison felt that the United States could delay no longer and still preserve its dignity and respect. Therefore, he labored all day on Sunday, January 24, on what might have become an historic "war" message. It was read on Monday in Congress and the members of the Committees on Foreign Relations of the two Houses, Democrats as well as Republicans, came to the White House for a conference. A declaration of war was apparently near at hand.

During this critical week-end the President of Chile happened to be absent from Santiago, else a reply would have

been received on Saturday. As it was, the reply came on Tuesday, January 26; Chile had bowed to the inevitable; apology and reparation followed. Blaine now drafted a note of acceptance and sent it to Harrison, who had also drawn up a draft. This he combined with Blaine's and returned it to him. "I liked your despatch better than mine," wrote Blaine on January 30, and ordered it put in cipher and sent. "The President feels a sense of relief, now that this Chilean affair is off his hands," wrote Halford in his diary on February 1. Doubtless Chile had counted on distance to nullify the greater power of the United States, but when Peru, Brazil, and Argentina gave support to the United States, Chilean resistance would have been foolhardy. Harrison's firm policy in dealing with the *Baltimore* and other incidents preserved the dignity and respect—and hence the influence—of the United States.

Some years later, when Harrison, then ex-President, was discussing the *Baltimore* incident with his intimate friend, Augustus L. Mason, he asked: "What in your opinion is the main function of government?" "The protection of the life, liberty, and property of its citizens," replied Mason. "Yes," added Harrison, "*wherever* they are."

The significance of American foreign policy as developed by Harrison, Blaine, and Tracy, was noted at the British legation, where Sir Cecil Spring Rice wrote: "the moral for us is: what will the U. S. be like when their fleet is more powerful, if the administration acts in a similar manner? I can't help thinking that serious difficulty may yet spring from the Behring Sea matter. . . . The danger is a real one, I think, but I can't say that it seems to be realized at home."

Two months after his ultimatum to Chile, Harrison was penning another terse, firm, diplomatic note, this time to England on the Alaska fur seal problem. Again the atmosphere in the Executive Mansion was electric. E. F. Tibbott, a member of Harrison's staff, wrote in his diary on March 22, 1892: "The President today finished his reply to Lord Salisbury's note refusing to renew modus in Bering Sea matter

and the outcome means a backdown on the part of G.[reat] B.[ritain], or war. Have been most of the day engaged in preparation of the reply and this evening took it in person to Mr. Wharton with instructions from the President that he deliver it to Sir Julian personally. This may be the beginning of a 'War' diary. . . ." England yielded, however, and events were set in motion which led to the conservation of seal life.

When Harrison left the White House on March 4, 1893, Hawaii "was casting a sunset glow over his dying administration"; the American navy ranked seventh among the navies of the world, instead of only twelfth, as it had when he took office; and the rank of the highest American diplomats abroad had been raised on March 1, 1893, from Envoy Extraordinary and Minister Plenipotentiary to Ambassador Extraordinary and Minister Plenipotentiary. As a result, at the Court of St. James's, the representative of the United States marched eighth in line instead of thirteenth. America, in full confidence, was appearing upon the world stage with a note of joyful achievement, feeling that she would dominate the century about to dawn.

Note.—This address was based largely upon documents found in the *Papers of Benjamin Harrison*, the *Papers of James G. Blaine*, and in the Archives of the Department of State. Copies of the extracts quoted from the diaries of Lt. Col. Elijah W. Halford and Everard F. Tibbott were given to the writer by these two faithful secretaries. The former burned his four-volume diary shortly before his death. The following books have been quoted: Robley D. Evans, *A Sailor's Log: Recollections of Forty Years of Naval Life*, New York, 1903; S. Gwynn, *Letters and Friendships of Sir Cecil Spring Rice*, 2 vols. Boston, 1929; and John Hay, *Letters of John Hay and Extracts from his Diary*, 3 vols., 1908; Alfred T. Mahan, *From Sail to Steam: Recollections of Naval Life*, New York, 1908. The sources of other materials have been indicated in the text.

AN ETHNIC MAP OF AUSTRALIA

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ABSTRACT

The political structure of the Australian aborigines is characterized by extreme simplicity. The largest political unit is the horde, a group of about 35 closely related individuals who occupy a well defined territory of varying extent over which they exercise complete autonomy. Since the pre-European population of Australia is estimated to have been between 250,000 and 300,000 there must have been between 7,000 and 8,600 independent political entities on the continent, an astonishing condition when compared with the aboriginal situation in other parts of the world. However, although the hordes lack political cohesion larger groupings are recognized and named by the natives on the bases of dialectic and cultural similarities and geographical contiguity. These larger units, which furnish a more practical basis for ethnological considerations, can be spoken of as tribes in spite of the fact that there is no semblance of centralized political authority nor any sense of political confederation. Altogether there were several hundreds of tribes, each consisting of a number of hordes which varied from a very few to several dozens, but the total number will never be accurately determined because of the lack of satisfactory information for many regions where the aborigines are now extinct.

To date very little attention has been given the question of arranging the vast quantity of material on tribes and hordes. The last attempt to compile an ethnic map for the continent at large was 50 years ago, when relatively little information was available.

This paper represents a preliminary attempt to give the evidence some semblance of organization with the hope that the numerous errors can be rectified, and the many gaps in information filled, by those who possess the requisite unpublished knowledge.

THE ethnic map herewith presented is intended to provide as complete and up-to-date a tribal map of Australia as is possible on the basis of available published sources up to 1938. The need for a map of this character has long been recognized for heretofore there has been none of continental proportions except that issued by Curr in 1887 when relatively little information was at hand.[1] Since by far the greatest

[1] Thorpe in 1913 compiled a list of 656 ethnic names but utilized only a very few sources. Although he equated a few cognates he made no attempt to treat his material in critical fashion or to distinguish tribes from hordes. His list

part of our knowledge concerning the aborigines has been collected during the past half century it is time that some effort be made to organize the vast quantity of data now available and to ascertain the gaps in existing information in order that inquiries to fill them can be made wherever possible.[1]

In order to understand the problems associated with the compilation and interpretation of the evidence on which the map is based it is necessary to discuss briefly the political structure of the Australians, an aspect of their ethnology which never has received more than passing attention in field studies.

Hordes

The political units in Australia are hordes, each possessing a name and consisting on the average of about 35 closely related individuals (7 to 10 families) who occupy a more or less well defined territory over which they have complete autonomy with prohibition against trespass by nonmembers. The hordes are patrilocal and, as the result of the prevailing cross-cousin basis of marriage whereby the proper mates automatically are not members of the same horde, also exogamous in a derivative sense. Political leadership of the horde is vested in a headman whose power is quite nominal for there are no political agencies whereby his commands, if he should make them, can be enforced. Although there seems to be a tendency for the position to be hereditary, unless a son obviously lacks the requisite qualifications, there are no indications of a development of any caste or class differences between the family of the headman and the other members of the horde.

Assisting the headman, and perhaps more important than he in several respects, is the council of elders which consists

was not accompanied by a map. In 1925 Roheim published a map with numbers representing 389 ethnic groups. The sole purpose of this map was to facilitate the plotting of the various culture traits in which he was interested.

[1] Several regional maps are available, such as those by Mathews and Howitt and more recently by Radcliffe-Brown, Elkin, Sharp, Stanner, McConnell, Tindale, Thompson, Warner, and others. However, in most instances these writers have not attempted to correlate older data which should not be ignored.

of the few middle aged and old men of the horde. In so far as their political prerogatives are concerned these elders seem to have little actual power and apparently influence the members of the horde mostly on the basis of their wisdom and ability to control public opinion. We are greatly in need of studies of the mechanisms of control and influence among the Australians, not only through the more formal aspects of political structure but also through the effects of dynamic personalities within the general political pattern. The indications are that the Australian system offers no marked opportunities of a political nature for ambitious individuals to gain power and prestige. Such outlets seem to have been restricted to social institutions, ceremonial life and ritual, and magic.

The number of hordes in Australia can be computed only roughly at the present time. It seems to be well established that these political units do not vary greatly in population, occasionally numbering less than 25 individuals, seldom more than 50.[1] Such a stability in numbers is in itself an interesting phenomenon which should be carefully investigated for, as the result of the greatly different ecological conditions in the various parts of the continent, the extent of territory required by a horde varies from as little as 20 square miles to as much as possibly 6,000 square miles. Thus the sizes of the territories seem to be adjusted to meet the requirements of a more or less standard and but slightly fluctuating population group regardless of the type of country inhabited. The boundaries of these territories seem to have been fixed for generations so that any change in ecological conditions is now compensated by adjusting the population to meet it, generally by infanticide. It therefore would be important to determine the factors which tend to make a population of about 35 individuals so consistently desirable in all parts of the continent.

As the result of the quite constant character of the hordes in so far as population is concerned it is possible to indicate an approximate number of hordes for the continent on the basis

[1] See Wheeler, Malinowski, Davidson, 1926.

of the estimated aboriginal population as of 1788, the time of the arrival of European colonists. The most satisfactory figures as determined by available evidence are those computed by Radcliffe-Brown which suggest that the aborigines numbered not less than 250,000 and probably as many as 300,000 or even more. The difficulties in arriving at any figure seem obvious. At no time has there been a complete census for any specific region and even for the better known tribes our knowledge of their vital statistics is almost nil. The estimates of the early explorers are most unreliable. Some in passing through well watered country happened to encounter few natives and therefore concluded that population was sparse, whereas others in relatively poor country, coming upon great numbers of aborigines assembled for ceremonies, grossly overestimated the normal number of inhabitants of the district. When due consideration is given the fact that the Australians are pure hunters and wild food collectors who, by necessity in some areas and by choice in others, live in small groups of closely related individuals in well defined territories off which they seldom wander except to attend ceremonies, it seems clear that reliable estimates of total population cannot be made by observation of assemblies but only on the basis of ecological conditions in each area.

The difficulties of making a satisfactory estimate for aboriginal times are further increased by the epidemic of smallpox which swept across much of Australia soon after the whites arrived on the coast. Long before most tribes had heard of Europeans, let alone seeing them, their numbers were severely decimated by this scourge. Thus when the first explorers penetrated inland they encountered populations already greatly reduced in numbers. Assuming 35 individuals to a horde on the average it would thus appear that there were between 7,000 and 8,600 hordes or completely autonomous units, an amazing political situation not found elsewhere in the world, at least in historic times, and seldom approached proportionately in even small regions of the other continents. On the following chart rough computations are given to show

the estimated population, the number of hordes and the density of population in the several regions of the continent where distinctive ecological conditions are apparent. These figures, extremely speculative as they may be in many instances, are nevertheless of value in that they serve to distinguish in a general way the areas of relatively dense population from those in which the natives were, or still are, comparatively few in numbers.

As can be seen in Figs. 1 and 2, which illustrate the chart computations, the regions most favorable to the aboriginal system of economy include in the apparent order of their importance (1) the tropical northern coasts and hinterlands of Queensland, North Australia and, to a somewhat lesser extent, of the Kimberley district, Western Australia, all of which are areas of little importance to the whiteman;[1] (2) regions now appropriated by the whites such as Victoria,[2] eastern New South Wales, southeastern South Australia, the eastern littoral of Queensland and the extreme southwestern corner of Western Australia, in all of which the aborigines are now virtually extinct; (3) areas now utilized principally for cattle and sheep, such as the northwestern portion of Western Australia and the inland central parts of North Australia, Queensland and New South Wales; (4) the desert country of the remainder of the continent, that is, most of Western Australia, western South Australia, all of Central Australia, southernmost North Australia and western New South Wales and Queensland.

A comparison of the density of aboriginal population in these various regions with the present density of white popula-

[1] Since it is undoubtedly true that population would generally tend to be somewhat denser in coastal areas than in the adjacent hinterland the liberty has been taken to indicate the likely difference between the coastal areas and the inland districts for this part of the continent. By comparing the map (Fig. 2) with the chart it will be seen that this procedure is purely arbitrary and not based upon facts for the divisions created. However, we should recognize some difference in density of population between the coastal and inland areas.

[2] For eastern Victoria the density is shown as one person to between 10 and 20 square miles but since the territory involved contains much sparsely inhabited mountain country the mean population per square mile for the entire region undoubtedly is much less than in the coastal districts where population was concentrated.

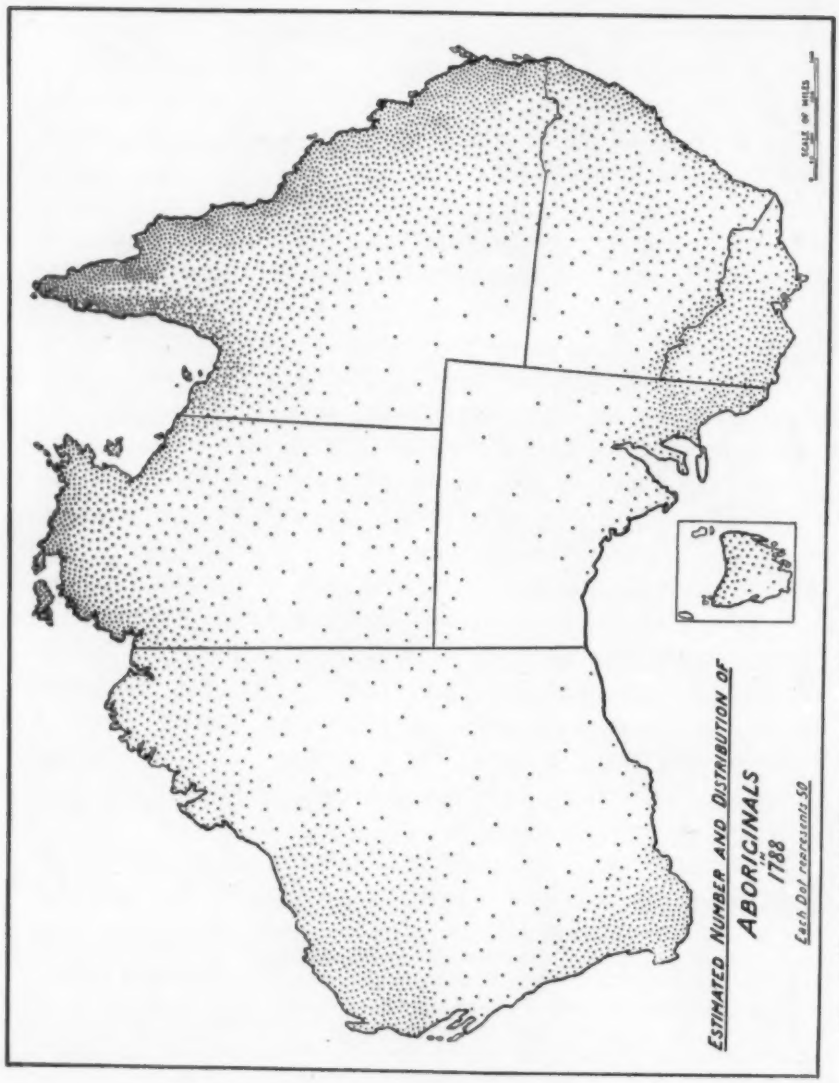

FIG. 1 (From Radcliffe-Brown, in Official Year Book of the Commonwealth of Australia, No. 23.)

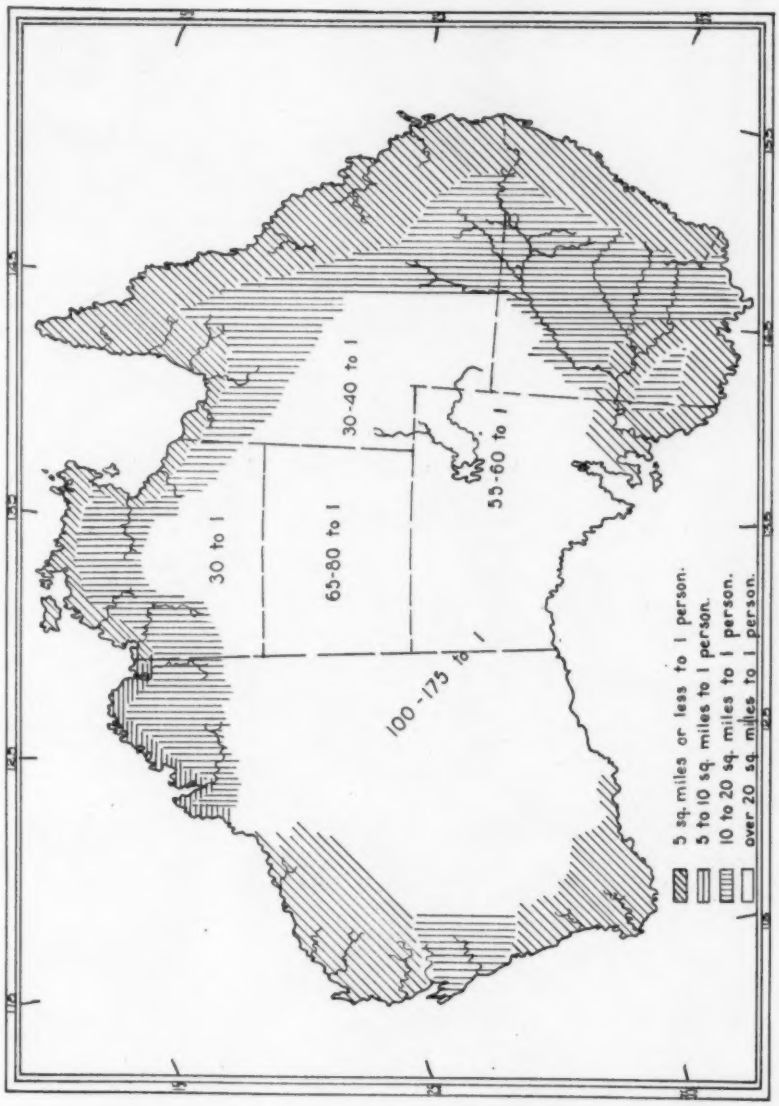
30 to 1
65-80 to 1
30-40 to 1
55-60 to 1
100-175 to 1
5 sq. miles or less to 1 person.
5 to 10 sq. miles to 1 person.
10 to 20 sq. miles to 1 person.
over 20 sq. miles to 1 person.

FIG. 2. Estimated density of population in aboriginal Australia as of 1788.

	Area	Estimated Population	Approximate Number of Hordes: Average 35 Persons	Approximate Density of Population per Square Mile
Central Australia	236,400	3,000– 3,500	85– 100	1 to 67 – 80
New South Wales				
Eastern	100,000	25,000– 30,000	700– 850	1 to 3.3– 4.0
Central-Western	210,372*	15,000– 18,000	420– 500	1 to 11.7– 14.0
	310,372*	40,000– 48,000	1,120–1,350	(1 to 6.5– 7.8)
North Australia				
Northern	187,220	29,000– 35,000	830–1,000	1 to 5.6– 6.4
Southern	100,000	3,000– 3,500	85– 100	1 to 30
	287,220	32,000– 38,500	915–1,100	(1 to 7.5– 9.0)
Queensland				
Southwest—Clarence River, N. S. W., to Broad Sound —to height of land	50,000	15,000– 18,000	420– 510	1 to 2.8– 3.3
Burdekin—Cape Rivers drainage—Mackay to Cairns	65,000	16,500– 20,000	465– 560	1 to 3.2– 4.0
Fitzroy tributaries, Dawson, Comet, Mackenzie Rivers drainage	45,000	10,000– 12,000	280– 340	1 to 3.7– 4.5
Cape York Peninsula to Gilbert River drainage	100,000	40,000– 48,000	1,140–1,360	1 to 2.0– 2.5
Central Queensland	180,500	10,500– 12,500	300– 360	1 to 14.4– 17.0
Western Queensland	230,000	8,000– 9,500	225– 270	1 to 24 – 29
	670,500	100,000–120,000	2,830–3,400	(1 to 5.6– 6.7)
South Australia				
Lower Murray Valley and adjacent areas	30,000	6,000– 7,200	170– 205	1 to 4 – 5
Remainder of eastern South Australia	150,070	2,500– 2,800	70– 79	1 to 54 – 60
Western South Australia	200,000	1,500– 2,000	40– 56	1 to 100 –130
	380,070	10,000– 12,000	280– 340	(1 to 32 – 38)
Victoria				
Gippsland	15,000	1,000– 1,300	28– 36	1 to 11 – 15
Murray River, east of Goulburn River	15,000	1,500– 2,000	42– 57	1 to 7.5– 10.0
Western Port District	20,000	3,000– 4,000	85– 115	1 to 5 – 7
Portland Bay District	10,000	3,000– 4,000	85– 115	1 to 2.5– 3.5
Northern Wimmera	10,000	2,000– 2,500	57– 70	1 to 4 – 5
Southern and Central Wimmera	17,884	1,000– 1,500	28– 42	1 to 12 – 18
	87,884	11,500– 15,300	325– 435	(1 to 5.7– 7.6)
Western Australia				
Northwest District, north of Gascoyne River	120,000	24,000– 29,000	685– 830	1 to 4.1– 5.0
Southwest District	50,000	12,500– 15,000	360– 420	1 to 3.3– 4.0
Murchison District	100,000	5,000– 5,500	140– 155	1 to 18 – 20
Kimberley	100,000	10,000	280	1 to 10
Central desert area	605,920	3,500– 4,000	100– 115	1 to 150 –173
	975,920	55,000– 63,000	1,565–1,800	(1 to 14.8– 17.7)
Tasmania	26,215	2,000– 2,500	57– 70	1 to 10.5– 13.0
Grand total	2,974,581	253,500–302,800	7,177–8,595	(1 to 9.8– 11.7)

* Inclusive of Federal Capital Territory.

tion (Fig. 3) reveals some interesting facts. Since it is generally assumed that the European system of economy fosters a much greater population than is permitted under strictly aboriginal conditions, especially when such a crude culture

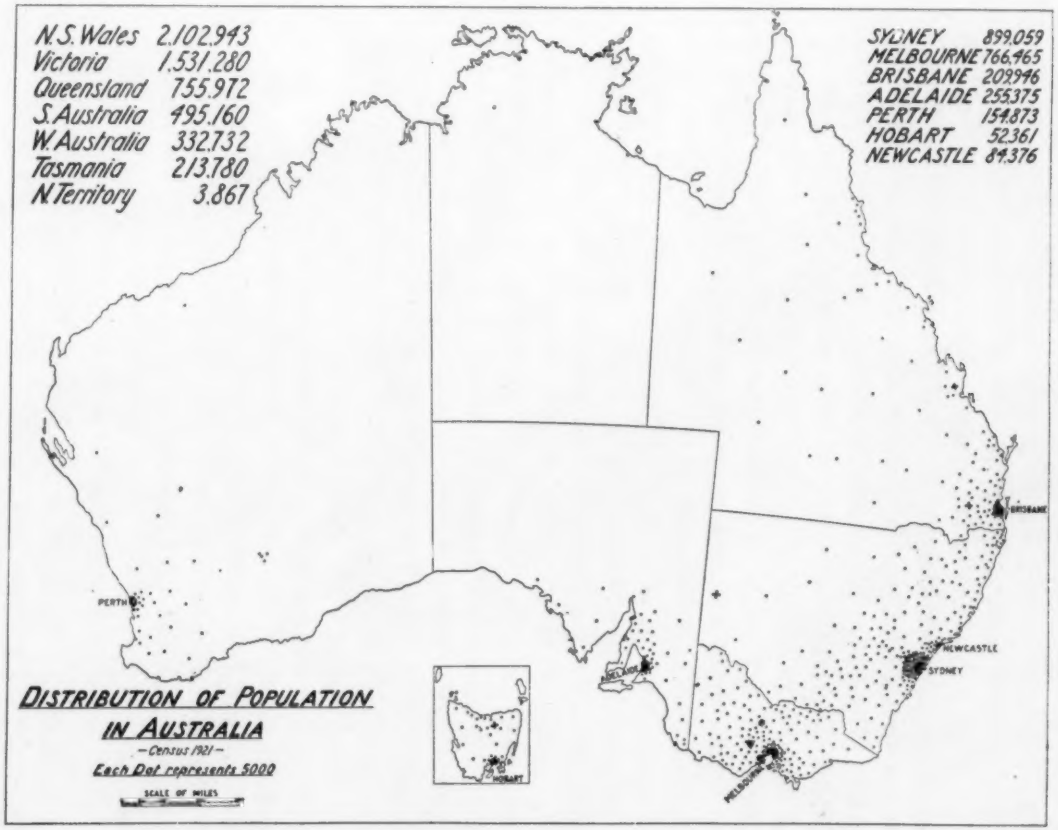

FIG. 3. (From Official Year Book of the Commonwealth of Australia, No. 23.)

as that of the hunting and wild food collecting Australians is concerned, it is important to note that this is true only for certain parts of the continent. Although the total European population of over 6,500,000 is more than twenty times the total native population of pre-colonial times, it should be noted that the former is concentrated in areas where native population did not reach its greatest density. The advantages of the European system in this instance are quite evident. However, in respect to the northern coasts and the interior deserts, the areas of greatest and least population density respectively under aboriginal conditions, the advantages are reversed. For example in North Australia and Central Australia combined, the aboriginal population apparently totaled between 35,000 and 42,000, but the white inhabitants in 1921 after about a half-century of penetration and settlement numbered only 3,867. Of these individuals most live in a few towns and the remainder are scattered over the cattle country. It thus seems clear that the European system of economy, in spite of great sources of food supply made available by domesticated animals, local or imported agricultural products, and various methods of preserving perishable commodities, is not always superior in terms of population supported in a given region. This situation is food for thought for those who favor a policy of forced acculturation of aborigines, whether through the activities of missionaries or governmental or other agencies, in those regions which have little or no economic importance to the whiteman and where the natives if left alone can exploit the natural resources of value to them in a relatively efficient manner.

Family Hunting Territories

Although the horde is the largest land owning unit it is by no means the smallest. In many parts of the continent the horde property is divided into several territories each owned by a family or an individual.[1] The boundaries are accurately delimited and generally are determined by natural

[1] For a more detailed discussion, see Davidson, 1928; Harrasser.

features of the terrain. Artificial markings are not unknown, but apparently are not common.

Trespassing upon family property in theory is strictly prohibited to all, but it has been reported that in practice this regulation often is not enforced in respect to near relatives or neighbors.[1] However, since all the males in a horde are closely related such statements may mean that all members of a horde come and go almost at will over all the family territories within the horde boundaries. We also have reports for some areas that although family territories are recognized they seldom are occupied exclusively by their owners who prefer to live with the other families of the horde and to roam in unison over the entire property of the group.

Ownership of these family or individual properties, in keeping with patrilocal tendencies, generally seems to pass directly from father to son. There also is evidence to show that in some instances a father partitions off a tract for a son during his lifetime or informs his family of the division to be made after his death. There also are several reports of aborigines giving or bequeathing land to friends.[2] Presumably the donors had no sons or made other provision for them. We have no detailed studies of the specific rules of inheritance in this respect but it would seem that there must be some regulations to prohibit the disinheritance of sons, for the latter could not appropriate lands elsewhere nor as a rule could they, as the result of the prevailing custom of patrilocal residence, take residence on the lands of their fathers-in-law. Similarly the rule of patrilocal residence seems to be responsible for the lack of inheritance of territorial rights by daughters, although some exceptions have been reported.

[1] For instance Nind, pp. 27, 28, 44, who wrote in 1831 and thus had excellent opportunity to observe the natives of southwestern Australia at a time when aboriginal culture was still intact, reports that members of the horde were accorded the privileges of breaking down grass-balls, digging roots and killing certain animals on the private property of individuals but were not allowed to fire the bush unless the owner were present.

[2] Collins, p. 599, was deeply impressed by Ben-nil-long "who both before he went to England, and since his return, often assured me, that the island Me-mel (called by us Goat Island) close by Sydney Cove was his own property; that it was his father's, and that he should give it to By-gone, his particular friend and companion."

The division of horde property among the constituent families has been described in brief for certain parts of Victoria, New South Wales, Queensland, North Australia, South Australia and southwestern Western Australia. Elsewhere it has not been reported. Although it probably is lacking in many areas it would not be surprising to learn of its presence in various other regions.

The first information on family hunting territories in the desert interior recently was collected by Mr. Norman B. Tindale of the South Australian Museum who kindly forwarded his data. His findings among the Pitjantara of the desolate Mann Range in northwestern South Australia show that each family owns a definite separate tract over which it has sole rights for the securing of dingo puppies, considered a great delicacy, during the months from July to October. These properties were said by the aborigines to have been the same for at least a generation.

There are several reasons why we should not expect typical family territories to be prominent in the desert regions. Not only is water confined to a few waterholes, often great distances apart, but in many of them the supply is not permanent. Hence the members of the horde must be free to move freely and extensively as necessity demands. In addition the exigencies of life in this barren area seem best met by the families living together, pooling their resources and sharing their food, for whereas one hunter may be successful one day he may search in vain for many succeeding days during which time he must depend for the sustenance of himself and family upon the good fortune of his fellows who in turn had partaken of the results of his own success. In the well-watered regions families living alone would never be very far from their neighbors but in the desert areas where between 100 and 200 square miles are required to support one person the segregation of families would cause virtual isolation.

Survivors of the Tangane (Tanganalun), now domiciled at Point McLeay, South Australia, reported that in aboriginal times family hunting territories were customary.[1] A family

[1] Information kindly furnished by Mr. Tindale.

tract was called *karawi* and its boundary (*kinara*) in some cases was marked by a stick on which had been cut marks to identify the owner. When an old man established a separate territory for a son by partitioning his property he marked off the *kinara* and the son thus became known as *kalyanan kinara*. It also was said that when a family became extinct its territory passed to the nearest family in the same moiety as the deceased owner. In the case of an owner who lacked sons, partitionment sometimes was made before his death by giving land to young men of the same moiety. These references to moieties should not be construed as meaning that the moiety, itself, had any control over these territories. Since a man's closest male relatives, his brothers and parallel cousins, automatically belong to his moiety, the disposal of land apparently was made to certain relatives who only incidentally were members of the same moiety as the owner.

Among the nearby Yaralde each family is said to have had two territories, one inland, one at the seashore. Names were applied to the individual tracts as well as to their boundaries.

Because of our scant knowledge concerning family hunting territories it is impossible to determine the criteria for their prevalence in some regions and their lack, or apparent lack, in certain other areas. On the basis of available information they seem to have been most common in the regions suited to European economy and now appropriated by the whites. This may be partially a coincidence but a detailed study of the ecology of the continent must be made before more detailed conclusions can be attempted.

Although a political map of Australia strictly speaking should portray the distribution of hordes, since they are the political units, it seems obvious that such would be impractical regardless of how desirable it might be. Even though we possessed the requisite information, which we do not, a map of extrordinary size would be required to permit the inclusion of several thousands of names. This difficulty is well demonstrated in Figs. 4 and 5 which show the distribution of hordes among the Karadjeri and Kurnai at opposite ends of the

continent. However, we have relatively little detailed information of this character, for very few investigators have sought information of a purely political nature. The reasons for this condition are not difficult to ascertain. Most

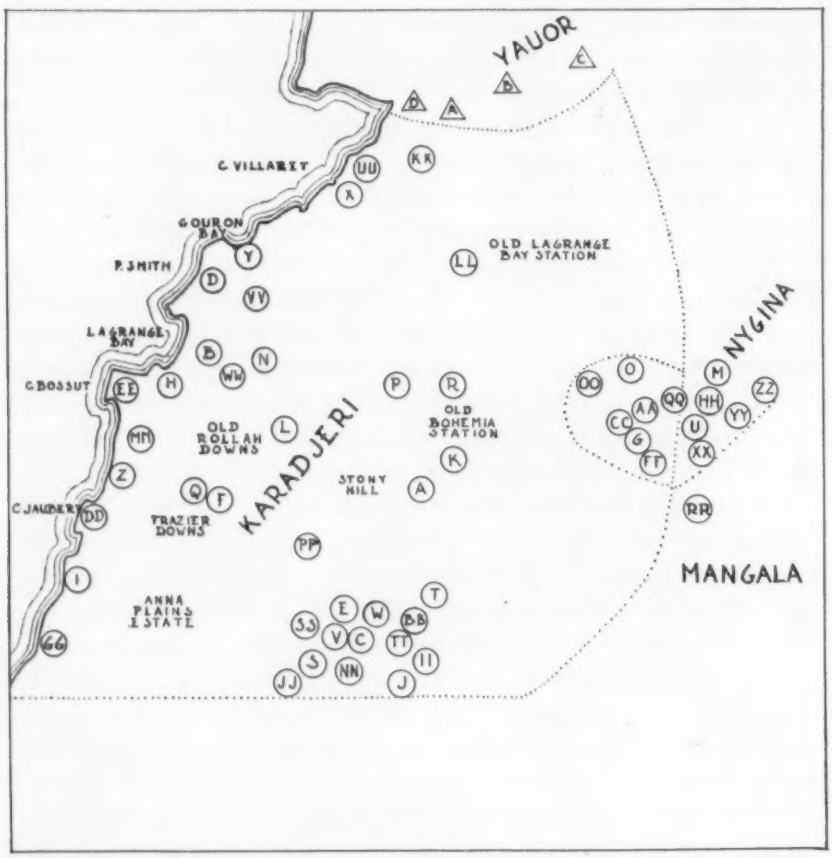

FIG. 4. Distribution of Karadjeri hordes including some of neighboring tribes (After Elkin).

Australianists have found the social structure of the aborigines so intricate that it has absorbed all their attention or interest. Furthermore the purely political aspects of the Australian culture pattern are relatively unimpressive and of local character when contrasted with social institutions

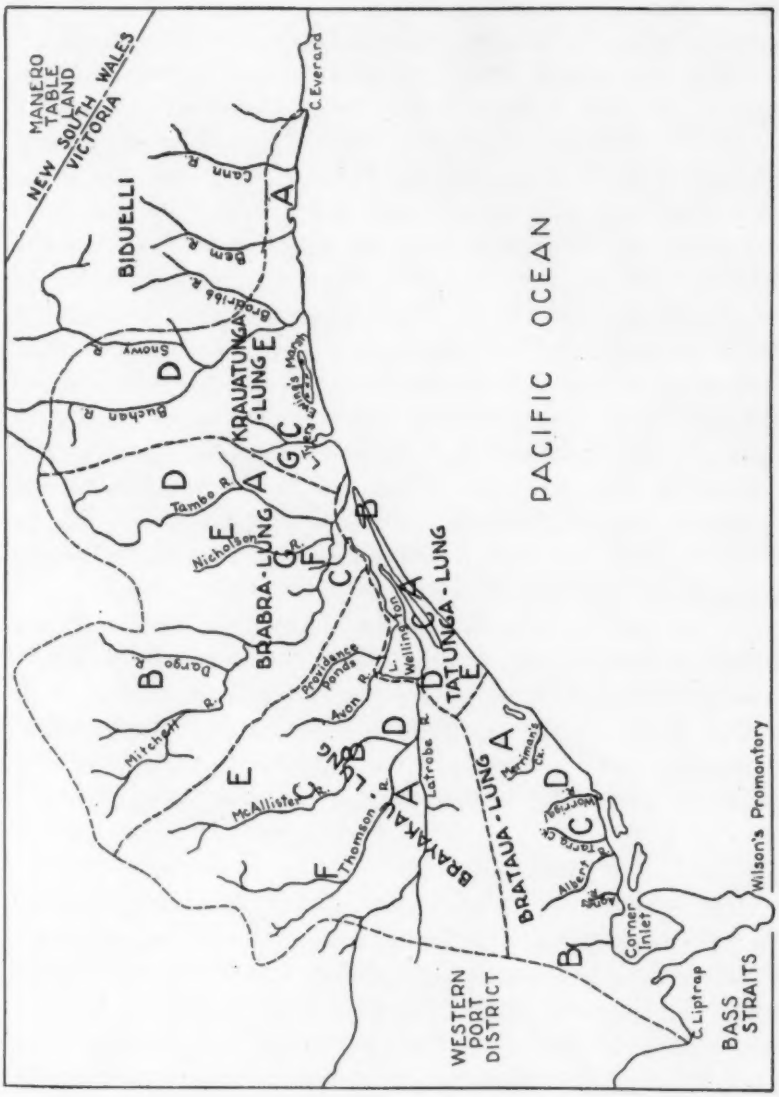

FIG. 5. Distribution of Kurnai tribes and hordes.

which constitute the dominent role in Australian life and individually are often of sectional or regional significance. Thus scores of hordes may be characterized by the same or similar social characteristics, such as kinship systems, moieties, sections, sub-sections, totemic rites, initiation ceremonies, and the like, and investigators who limit their studies to these aspects of ethnology can ignore the details of local political structure without serious difficulties.

In addition it should be pointed out that there has been no necessity to determine the names and boundaries of the political units of the continent from the point of view of government authorities. Although the horde is the fighting and land owning unit its limited population and primitive weapons prohibited it from offering any effective resistance to European colonization. There have been no wars between the whites and the aborigines, in the sense of the word understood in the Americas and Africa, but at the most only local skirmishes with police. Although Australia can well be ashamed of its methods of annihilation of the aborigines in certain areas it should be recognized that European diseases and the insidious forces of acculturation have taken perhaps a greater toll of native lives than have firearms. At any rate it has seldom been necessary for the government to deal with political units for the purpose of defining boundaries or of purchasing native lands, hence the lack of government interest in recording information about aboriginal political organization.

Tribes

Although there are no political units greater than the hordes it is possible and desirable to apply the term tribe in a non-political sense to groups of hordes which are recognized by the natives, themselves, as cultural, dialectic and geographical units, each with a name. The Australian tribe thus differs from tribes in other parts of the world principally in that it lacks a centralized government. It is not a confederation for there is no political cohesion between its constituent hordes. There is no tribal chief nor even a tribal council with the result

that each horde retains complete political autonomy. Nor are there any defensive or offensive alliances between the individual hordes. Were the Australians politically minded it would seem that this situation would offer unlimited opportunities for the ambitious individual interested in military conquest and land aggrandizement. However, Australian culture does not offer a setting in which the politically minded could prosper for the entire philosophy of life of the aborigines is contrary to such possibilities. For religious reasons each individual, at least in much of the continent, feels an attachment to the land of his birth whence at a certain sacred spot his spirit emanated and to which it will return at the time of his death. To natives imbued with this philosophy land in other localities, where there would be a normal complement of human beings with souls associated with the local sacred spirit abodes, would be entirely useless to those who lacked spiritual connections therein. In this respect it is important to note that there seldom has been any mass exodus by aborigines from lands appropriated by the whites. The survivors of the initial period of contact, peaceful or otherwise, usually have remained until they became extinct or so decultured that they offered no serious objection to eviction and settlement on some reserve. Another example of the Australian's attachment to the land of his birth is found in the reports that aborigines may refuse to act as guides beyond certain points, at least partially on the ground that they do not belong to the territory beyond.

The presence of so many small political units in Australia has necessitated the development of some recognition of the rights of neighboring hordes in order that there may be mutual advantages. Thus we already find in Australia what might be called the beginning of international law. In addition to the recognition of territorial soverignity we find the sacredness of messengers and envoys, rights of asylum, domicilement and hospitality, and the exchange of economic goods over wide areas, in some instances on the basis of individual contract whereby one individual orders the manufacture of certain articles from a business associate in a nearby or distant horde.

Whether the Australians in certain regions were in process of developing larger political units is a moot question. In some areas, such as for example the Lower Murray River Valley, it might seem at first glance that there is or was the beginning of some extended political authority. Such appearances, however, must not be confused with the great assemblies for social ceremonies reported by early writers which often were attended by hundreds of natives who temporarily were well organized for the purposes then at hand under capable religious and ceremonial but non-political leaders. It would seem that only by a complete shift in emphasis in the Australian culture pattern would it have been possible in the areas of relatively congested population to have welded the hordes into larger political units. There seems to have been no general tendency in this direction.

The number of tribes in Australia has not yet been satisfactorily determined. Some exceptional tribes are known to include a few dozens of hordes whereas others embrace only six or eight. Our information at present is insufficient to indicate what on the average the number of hordes to a tribe may be. Undoubtedly the number varies considerably in the different parts of the continent and in terms of density of population. If we could assume that in the congested areas a tribe would include at least ten hordes it would follow that there are between 700 and 860 tribes for the continent at large. The latter figure coincides approximately with the number of names on the accompanying tribal map but this cannot be accepted in itself as proof that the map is approximately accurate either in respect to the names themselves or in the distribution as given. Undoubtedly there are many more names to be added. On the other hand, it seems quite certain that many of the names included may not represent tribes but may be those of hordes, localities, geographical features or dialects. Much of the information available in the great number of published sources from which the tribal map was compiled contains ambiguities and conflicting evidence.[1]

[1] See Davidson, 1938.

Hence of the several thousands of names recorded it often is impossible to come to any definite conclusion as to just what they represent. However, the percentage of names concerning which there is uncertainty is relatively small for, aside from cognates, of which there are many, most of the names are specifically stated to represent tribes or hordes. It is of value to note that the distribution of names on the large map tends to coincide in general with the maps of population density (Figs. 1 and 2).

The specific problem of accurately locating on the map the various tribes is a most difficult one. In only a few instances have boundaries been described and, as a rule, only approximate limits have been given. In the few cases in which they have been placed on maps the boundaries usually are shown by straight lines rather than by lines which follow the local topographical features. The extent of most tribal territories, however, is not even approximately defined in the reports but the locations are given roughly in terms of prominent landmarks such as mountains, rivers, bays or other recognizable features, or in terms of orientation from them. Thus a tribe may be said to inhabit the Mount A district, the region northwest of Mount B, the shores of Bay C, or inland therefrom, or the central portion of River D. Distances usually are not included although occasionally we are told that a tribal area is small, considerable or large, whatever these terms may mean. Obviously a "small" tribal territory in the desert may be many times as extensive as a "large" area in some of the better regions.

Unfortunately, too, the authorities do not always agree. Some tribes are reported by different authors in various locations within a given region. When these locations do not conflict with other tribal lands an extensive domain for that particular tribe possibly may be indicated. In many instances, however, parts of this region may be assigned by other writers to different "tribes," although in reality the latter may be constituent hordes. In instances of this kind information usually is not sufficiently detailed to warrant an assumption as to just what the situation may be.

Since many of the earlier writers received their information by correspondence from distant settlers, who in turn may never have visited many of the tribes they reported on, but knew of them only through hearsay from local blackfellows, most of these conflicting reports of a tribe's location may be erroneous. Here again, however, it is impossible to make certain which accounts are reliable. Nevertheless the placing of these names on the accompanying map required the exercise of arbitrary judgment on the part of the author with the probability that he has accepted as authentic many claims which are faulty and has underestimated other evidence which deserves greater weight.

In attempting to secure the most accurate results the author has followed as consistently as possible the policy of accepting as the most reliable the reports of professionally trained fieldworkers or of others known to have visited the tribes in question. The least weight has been given to the statements of those early writers who derived their information from distant correspondents. Such a policy obviously involves inaccuracies and injustices for many of the correspondents of Curr, Howitt and others undoubtedly were careful observers in respect to such matters as tribal locations and, on the other hand, professional fieldworkers often do not visit all the tribes presented on their maps but rely upon native informants for data concerning them. Physiographical maps for many parts of Australia furthermore do not include detailed landmarks of importance to the natives and may give only English terms which confuse them. Hence many aborigines can not indicate on the white man's map the location of geographical features well placed in their own minds which they would have no difficulty in finding.

Another difficulty familiar to every cartographer is the confining of long names to small spaces. The names of many Australian tribes and hordes contain several syllables and, as has been indicated, in some regions the territories occupied may be relatively small. In the few cases where this is known to be so the names have been run off the map when possible or indicated on the smaller sectional maps (Figs. 6 and 7).

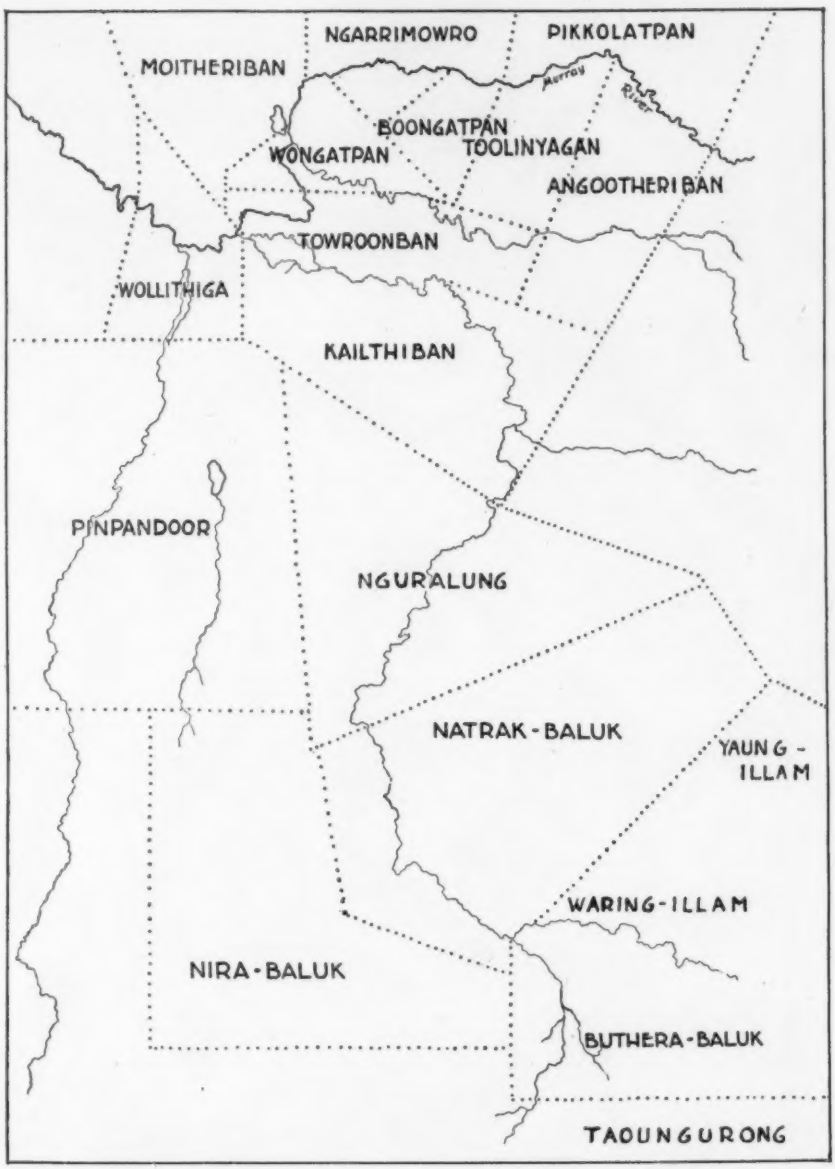

FIG. 6. Distribution of tribes in northern central Victoria and adjacent New South Wales (After Curr).

FIG. 7. Tribes of Southwestern Victoria (After Smyth).

However, there are numerous instances in which the extent of territory occupied by a tribe is not given in the source material, hence it is impossible to determine whether the space alloted the printed name extends over and beyond the actual boundaries of that tribe. In addition it has been necessary many times in respect to the congested tribal areas to fit the printed names into the spaces available. Thus the lettering may be horizontal, vertical or diagonal and may give the impression that some tribal territories are long narrow strips extending a great distance in one direction but not in another whereas such expanses cannot be affirmed and should be regarded as possibly if not probably extremely distorted.

Stability of Tribal Territories

The tribal distribution as shown apparently represents the status quo for an undeterminable but considerable period. There seem to have been no major movements of population in late prehistoric times nor are there any obvious indications that such have taken place at any time in the past. Australian traditions not only do not emphasize migration legends for political or ethnic groups but for the most part are quite deficient in them. On the other hand, tales which relate the movements of *mythical* ancestors are quite common in certain areas where each totem group has its own legends of the wanderings of the *mythical* beings responsible for its beginning. Particularly in Central Australia have these tales been abundantly recorded, but these *mythical* wanderings are concerned with the establishment of religious totemic centers and are not intended as, nor can they be interpreted as, tales of tribal migrations.

It is possible that intensive inquiry in the field might elicit information of ethnic movements. Off hand, however, we have no reason to believe that there have been any major migrations within recent centuries. Occasionally one meets with traditions of minor shifting of peoples such as the Dieri claim that their forefathers, whatever generation this term may imply, formerly occupied the territory in which the Wonkanguru now dwell. The latter, it is said, drove out

the Dieri, because they had been expelled from their own country by the Wonkamala.[1] How much truth there may be in the details of these claims we have no way of knowing. We have no reason to doubt that some shifting may have taken place for, if we may judge from local climatic conditions in historic times, the region concerned is subject to such severe changes in the course of a few generations that the density of population must have varied greatly over a long period of time. Ecological conditions, therefore, may have encouraged some movements of local character as the climate fluctuated but such adjustments may have been very gradual. Thus we should not accept Howitt's intimation that the Dieri and other tribes were forcefully ejected from previously occupied lands without better evidence that such was the case. Our reasons for suspecting that warfare may not have been necessarily the cause for such a change are to be found in the Australians' complete lack of interest in land aggrandizement and in our knowledge of what has happened in adjacent regions to the west in the last few decades.

An interesting shifting of population has taken place in recent years in western South Australia and adjacent Central Australia and Western Australia, a tremendous arid area which apparently constitutes the most inhospitable part of the continent. It is indeed impossible for Europeans, or for that matter for aborigines from other parts of Australia, to comprehend the rigors of existence in this region. The waterholes on which depend all living creatures are few and separated by vast expanses of desert, rough limestone outcroppings, desolate mulga country or barren mountain ranges. Animal life, as might be expected, is most meager and edible wild plants, so important as supplements to a preponderantly meat diet, occur only sporadically and never in great abundance. More important still, the region is subject to such severe droughts that even the normal paucity of sources of food cannot be depended upon with any degree of security. Here then is an area which requires of its inhabitants a most

[1] Howitt, p. 45.

intimate knowledge of natural history and geography, a constant vigil for game, an infinite patience, the keenest exercise of ingenuity, and the physical stamina to meet the vicissitudes of life. The aborigines have succeeded in living here only because they have adopted the sanest methods within their knowledge of adjustment to a stern and exacting environment.

It must not be supposed that the aborigines of this region regarded their struggle for existence as other than normal. The neighbors of each horde live under approximately the same conditions so that the relative ease of existence in other parts of the continent is unknown. In their experience life is easy in good seasons and difficult in years of drought. Hence under aboriginal conditions there were no incentives to leave these homelands and migrate into surrounding regions already supporting a full complement of human beings. But even if the thought of migration had occurred it would have been physically disastrous to attempt to wrest from other hordes of equal strength the lands they would die in defending.

The main factor in the struggle for life is the maintenance of population at or below the saturation point and this course has been successfully followed by the practice of infanticide, a widespread custom in Australia. In good years babies could be permitted to live providing the mother was not overburdened by older infants still dependent upon her, but in drought years the babies perforce had to be killed. Thus in aboriginal times the population must have fluctuated within certain extremes in accordance with economic conditions and to have remained quite stationary geographically. Confined to its traditional territory by cultural and physical forces each horde has tended to maintain the status quo and to solve its problems of subsistance by altering its numbers to suit the supply of food available.

The arrival of Europeans in Australia in no way changed the conditions of life in western South Australia, which never was colonized, but their coming into adjacent regions has provided the natives of the western portion with new solutions

to their problems. About a century ago the whites commenced their penetration of South Australia and gradually worked north from Adelaide until cattle stations were established in all of the suitable pasturage country northwest and west of Lake Eyre. Indeed the rapid growth of the colony and the general enthusiasm of the times induced many individuals to settle even marginal areas where the grass supply was satisfactory only during good years. Drought conditions were not serious during these early years, although they may have appeared so at the time, and a most happy future was confidently anticipated. The colonists prospered, their wealth accumulated and apparently to their satisfaction the aborigines in the settled regions dwindled so in numbers that they ceased to be a factor of danger.

The colonists hardly had arrived in much of the occupied territory, however, when climatic conditions began to change gradually for the worse. Droughts not only increased in frequency but also in duration. It is said in South Australia that the early settlers used to complain if they had three poor years in ten but that at the present time the inhabitants inland are overjoyed if they have three good years in a decade.

As a direct result of these droughts the cattle area has been considerably reduced in extent. Cattle have been shot by the thousands in order that the limited water supply might be sufficient for the living. Many homesteads and thousands of square miles of territory have been abandoned. However, although this land is regarded as worthless under the European system of economy it nevertheless is very desirable country from the point of view of the natives of the nearby desert area and, since the aborigines of the abandoned region have become decimated to the extent that they no longer can occupy it fully, or are so accultured to European institutions that they do not care to return to a purely hunting existance, a large area capable of supporting hundreds of blackfellows has again become available. This vacuum has made a strong appeal to the nearby desert-dwellers who faced with apparently unprecedented hardships as the result of the severe

drought conditions in recent years have gradually moved eastward as the whites have withdrawn.

The threat of extermination under which these aborigines have lived during the past few decades is well illustrated by the great reduction in their population in the last century from an estimated 2,000 to an estimated 900 (Elkin) or one person to 175 square miles in 1931. Although this decrease cannot be attributed entirely to drought conditions the latter certainly have contributed in large part to this reduction. For instance in 1930 in a group of 60 natives met by Colson in the distant Petermann Range (Central Australia) no children under four years of age were seen.[1] Presumably all babies during the previous four years had been killed and, since the Australians are a practical people, also eaten. To the sentimental European such a procedure is unthinkable but the Australian knows from centuries of experience that the perpetuation of the group depends primarily upon the knowledge possessed by the older folks who have lived through various drought periods and that this must be depended upon if the group is to be protected from complete extinction. Any attempt to save the infants, the most dependent class and the only one easy to replace, might result in the destruction of all.

The movement into the abandoned cattle country apparently has not taken place by wholesale migration but rather by the slow but constant infiltration of small groups of natives, perhaps a family or two at a time. However, it appears that over a course of several years whole tribes have gradually shifted their center of population, the land which they have abandoned thus becoming available for their neighbors. For instance I have been informed by Mr. Tindale that the Pitjantara of the Mann Range have extended their area into the western Musgrave Range formerly inhabited by Jankundjadjara who have moved eastward into the old station country.

A similar movement also has taken place westward toward Laverton and Mount Margaret in Western Australia where

[1] Elkin, p. 64.

the original tribes for various reasons are virtually extinct. Dr. Elkin in 1930 and subsequently Mr. Tindale found Mandjindja from the Warburton Ranges, near the South Australian border, as well as natives from the surroundings desert country, working their way westward toward the area of white settlements.

Another factor attributable to the whites in the expansion of the desert dwellers is the establishment of missions and government reserves in Central Australia (Hermannsburg) and southern South Australia (Ooldea, Koonibba and various settlements along the Bight). As an initial result the local tribes in each area, attracted by free food, tended to congregate at these points during certain seasons and to return to their properties for hunting when this activity was profitable. In southern South Australia in particular, however, these visits gradually increased in duration until in some areas the natives have become entirely dependent upon the whites and virtually have ceased their efforts for livelihood by aboriginal means. Thus more lands have become available for the more distant tribes which during the recent severity of the drought have extended their hunting areas southward and northward as well as eastward and westward. In addition natives from distant regions have been prompted to make visits to the missions and reserves and many of them, attracted by the glamor of European institutions and by a constant although not a varied food supply, are now satisfied to remain close to the seat of benefaction. Even Pitjantara natives of the Mann Range have made their appearance at Hermannsburg, Central Australia, but return home to hunt when the rains have watered their country. At Ooldea, South Australia, natives are found from the distant Everard, Musgrave and even the Macdonnell Ranges, 235, 280 and 470 miles respectively to the north.[1]

The recent shifting of population in western South Australia and adjacent areas thus seems to have been abetted by the withdrawal of Europeans. However, it is important

[1] Bolam, p. 71.

to note that the changes noted have been gradual. Extensive lands for re-occupation by aborigines did not suddenly become available, hence the population adjustment apparently has proceeded without any important disruption in native culture. It seems permissible to suspect that similar droughts may have prevailed in pre-European times and that the return of better conditions may have encouraged aborigines of neighboring regions where the population had not been so seriously curtailed to infiltrate areas where the number of occupants had been drastically reduced. Such a movement may have taken place in the form of a slow settlement of families on lands belonging to distant relatives, or possibly by marriage with matrilocal residence. It would be very important to learn the details of the recent adjustments reported for South Australia by securing case histories of families and individuals who have changed residence. Inquiry should be devoted to the collection of complete data in respect to the relationships between and the totemic affiliations of the present and former occupants of specific territories.

It seems to be significant that the few shifts in local populations of which we have record seem to be associated with the return of better conditions and not with the rise of drought conditions. Thus reduced food supply does not appear to be a cause of movement, at least not an important cause, for replacements in population for those who die each year can be checked through infanticide as the potential supply of food diminishes. We cannot doubt that many natives in South Australia and Central Australia would have starved to death during 1920–1930 if rations had not been made available at government reserves and mission stations. However, the serious threat to life over a period of many years seems to have not prompted any of the local hordes to become marauders on the lands of others or to have attempted to secure territorial expansion by appropriating or by attempting to seize the lands belonging to their neighbors.

Incidentally it is worth noting in passing that those aborigines who have partaken of the whiteman's charity in

many instances have saved their bodies but have lost their souls. Those who for one reason or another have lingered near the centers of benefaction are now suffering all the maladjustments which usually accompany rapid cultural change. In partially adopting European culture in which full opportunities for economic and social assimilation are not granted, many have developed an inability or an unwillingness to return to their previous ways of life and unknowingly are destined to an existence of dependency on charity or professional mendicancy.

Aggregations

In much of the older literature on the Australians one finds mention of "nations." In some instances the authors have regarded hordes as tribes, hence the meaning of nation to them corresponds to our use of the term tribe. Other writers, however, employ the word nation to signify a larger body consisting of several tribes. In view of its modern and historical connotation, particularly in a political sense, the word nation obviously is unsatisfactory for such a use. Aggregation, therefore, seems much better suited for the purpose involved.

As the result of the varying and often questionable criteria used by several writers the aggregation is not readily definable. It may imply a linguistic unit consisting of several tribes whose dialects are quite similar, a geographical unit of several tribes who occupy a distinct region, or a cultural unit in which a number of tribes possess in common some prominent cultural pecularities. However, since there is no specific correlation between language, geography and culture, only one can serve as the basis for classification. Furthermore, particularly in respect to culture, the conditions are constantly changing. For practical purposes it would be convenient to group the several hundred tribes into a few dozen aggregations on the basis of cultural evidence and to classify the many dialects into a smaller number of linguistic units. But to accomplish these tasks much more information

is required than is available at the moment. In addition it should be realized that such classifications would be merely for purposes of our own convenience, not recognized by the aborigines who do not think politically in terms greater than the horde, linguistically in terms greater than the dialect, or culturally in terms greater than the provenience of the institutions of direct concern to the members of the horde as revealed by their own experiences. It should be emphasized, however, that aggregations, like tribes, are not political groupings in any sense whatsoever, but conceptualized units which facilitate our handling of Australian data.

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THE CORPUS VASORUM ANTIQUORUM

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(*Read April 23, 1938*)

ABSTRACT

This paper is in the nature of a report of progress by the Committee of the American Council of Learned Societies, which has charge in the United States of the *Corpus Vasorum Antiquorum*. In this publication it is planned ultimately to record all ancient clay vases from Europe, the Mediterranean area, and the Near East. It was one of the first collaborative projects sponsored by the Union Académique Internationale after its organization in 1919, and already some fifty fascicules have been brought out, based on collections in Belgium, Denmark, France, Great Britain, Greece, Holland, Italy, Jugoslavia, Poland, Spain, and the United States. Six fascicules based on American collections have already appeared, and others are in preparation.

To illustrate the varied interests which such a publication may serve seven plates, drawn from the American fascicules, are discussed. They illustrate many different types of Greek pottery, but emphasis is laid on the subjects treated, and the manner in which they serve to illustrate several aspects of life in antiquity.

THE *Corpus Vasorum Antiquorum* is a project of the Union Académique Internationale, sponsored in this country by the American Council of Learned Societies; and this paper is in the nature of a report of progress by the Council's Committee in charge of American participation. In these days, when international relations seem to be in such a hopeless state, it is pleasant to describe one undertaking in which scholars of many nations have collaborated with a minimum of misunderstanding and no real friction since its inception in 1920. No small part in this happy result is due to the tact of the first director, Monsieur Edmond Pottier. Thanks to the real affection which all archæologists felt for that distinguished scholar, the plans for the Corpus were so far advanced at his death in 1934 that, barring a complete collapse of international relations, it seems destined to pursue its course undisturbed. Already over fifty volumes, devoted to collections in eleven different countries, have been published, and every year sees

several additions to the list. In a sense, no doubt, it is an endless job. The plan contemplates the publication, with at least one illustration and a brief description, of every ancient clay vase from Europe, the Mediterranean area, and the Near East. The number of the vases, estimated in 1920 at something over 100,000, is every year increased by the results of excavation. A survey made in 1929 by Professor H. R. W. Smith of the University of California brought out the surprising fact that there are in the United States alone over one hundred and seventy collections in which there is material which should be included. Some of these, of course, are small, but the number gives an idea of the magnitude of the undertaking.

The American collections so far published include a volume devoted to two private collections, the Hoppin and the Gallatin (the former now in the Fogg Art Museum of Harvard University), a volume for vases in the Rhode Island School of Design at Providence, one each for collections in the University of Michigan and the University of California, and two for the Robinson collection at the Johns Hopkins University. Definite arrangements for the three largest collections in the United States, those of the Museum of Fine Arts in Boston, the Metropolitan Museum in New York, and the University of Pennsylvania Museum, have not yet been made, but the authorities of all three have expressed their interest in the plan, and ultimately will publish a part, at least, of their treasures in the Corpus, perhaps reserving certain groups that have been published or are to be published in a more elaborate form.

The interest in a publication of this sort is, of course, primarily archæological. The authors tend to emphasize the attribution of vases to centers of production and individual artists. But the Corpus serves other purposes. The vases are a mine of information in regard to many aspects of ancient life, and the rest of this paper is devoted to showing a few examples from the American volumes already published and commenting on their less technical aspects.

FIG. 1. Gallatin Collection. Plate 4.

The first example (Fig. 1) is taken from the Gallatin Collection, and shows two vases of Attic black-figured ware, painted in the style which was in vogue during a large part of the sixth century B.C. On the amphora at the top is depicted one of the adventures of Herakles, the favorite hero of the black-figure period. He has captured the Erymanthian boar alive, and with it threatens Eurystheus, who has taken refuge in a great storage vase sunk in the ground. Athena, Herakles's patron, looks on with evident approval. Opposite her, to balance the composition, is a draped female figure, probably to be identified simply as a servant of Eurystheus. On the reverse is a well designed scene from daily life, with a fully armed warrior and an archer as a central group, flanked by two elders. The second vase on this plate shows an interesting variant on another familiar type, the warrior's departure. A woman pours a parting drink into the cup which the warrior holds. On the other side an archer holds up a bone before the female dog which accompanies the warrior, and she in exasperation gnaws at the skin covering of his quiver, one of those individual touches which add interest to an oft repeated scene. On the reverse we have a dancing mænad between two Sileni. Both vases show the careful distribution of the figures, the skillful use of the engraved line for details, and the restrained use of white and purple over-color, which add much to these products of the archaic art of Attica.

The second plate that I have chosen (Fig. 2) reproduces four oil flasks or lekythoi in the Rhode Island School of Design, which are typical products of the Attic potters of the early fifth century. These are in the so-called severe red-figured style, in which figures were left in the color of the clay and the black glaze was employed for background. Each has its special interest, no. 1 for the unusual association of Hera with a falcon and the well drawn chair and cushion, no. 2 for the incense burner which the flying Victory clasps in her left hand, no. 3 for the unusually large bird which the woman holds (perhaps a quail), no. 4 for the use of a single animal as a subject. This is uncommon in such vases, in which the human interest usually prevails.

FIG. 2. Rhode Island School of Design. Plate 19.

1a
[1 : 2.1]

1b
[1 : 2.1]

FIG. 3. University of California. Plate XL

My third example (Fig. 3) I chose primarily on æsthetic grounds. The plate is based on a deep cup in the University of California from the end of the severe red-figure period, that is, about 460 B.C. On one side a boy, whose lyre suggests that he has come from school, is offered a strigil, or scraping iron, by an elderly admirer, while on the other a similar offering is made to a boy who has just stripped for exercise in the palestra, suggested by the goal post behind him. Such scenes from daily life are special favorites with a whole group of painters in this style. No small part of the charm of the group depends on the very skillful use of ornamental patterns, which were always, apparently, painted without any use of stencils or other guides.

Next we may look (Fig. 4) at a slightly later work in the Hoppin Collection, dating close to the middle of the fifth century. This, too, is red-figured, painted in the manner often called the large style, because the artist uses a great part of the surface at his command for compositions of a few figures on a large scale. The central figure is Dionysus with his thyrsus, accompanied by two satyrs and two mænads. The details at the top show well the striving for expression which characterizes many works of the period. The large style is generally believed to reflect the mannerisms of the great fresco painters of the fifth century, especially the famous master, Polygnotos.

The next vases take us into another group of works—the Panathenaic amphorae which were given as prizes in the games at Athens. A series of these prize vases has been preserved, mostly of the sixth and the fourth centuries. They were always painted in the black-figure technique, and follow a fixed scheme, with Athena on one side and athletes on the other. Figure 5 represents one in the Hoppin Collection and illustrates the fourth century type, which is taller and slenderer than the sixth century examples, with an archaizing and much elongated Athena. It can be dated exactly in the Attic year 340/339 by the inscription of the archon Theophrastus. Though it conforms in general to the type of the

FIG. 4. Hoppin Collection. Plate 14.

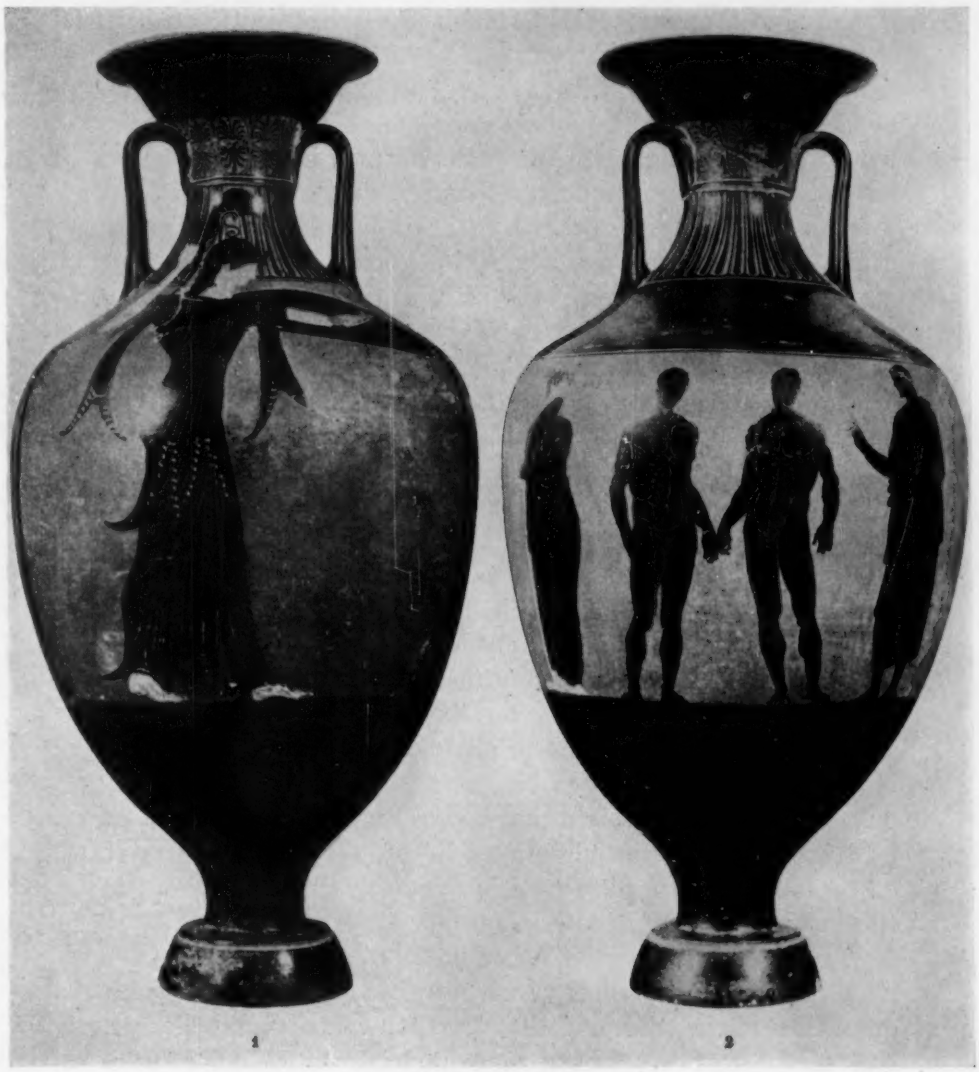

FIG. 5. Hoppin Collection. Plate 6.

Panathenaic vases, it presents one unique feature in that the two boxers on the reverse are accompanied not only by a trainer, who seems to be admonishing them, but also by a female figure. The inscription ΟΛΥΜΠΙΑΣ shows that she is Olympia personified.

Because until quite lately none of the Panathenaic vases could be surely dated to the fifth century, it had often been argued that the prizes during that century, or part of it, had been of a different sort. But in the Robinson collection there are four examples for which strong arguments for a date in the last quarter of the fifth century can be advanced. Figure 6 shows both sides of the best preserved of these, and the reverse scenes on two others. The argument for a fifth century date is based on the shape, which is midway between the squat examples of the sixth century and the more slender type of the fourth, on the use of Ionic letters in the inscriptions, and on certain similarities in the drawing of the athletes to that on red-figured vases of the decade 420–410 B.C. Two of the Robinson vases have a further interest in that the boxers who here appear are clearly youthful, and contenders in the boys' contests are rarely found on a preserved Panathenaic vase. The runners on the fragmentary third vase are obviously young men, and on the fourth (not represented here) they are adults. Since all four vases were found in a single tomb, Professor Robinson has made the interesting suggestion that all might have been cups won by a single individual, who was victorious in the boys' wrestling in 422 B.C., in the running race for young men in 418, and in the men's wrestling in 414.

The next two vases belong to a still different category, the Athenian white lekythoi, which were manufactured in great quantities in the second half of the fifth century and the first half of the fourth. These were made principally for tomb offerings, and one of the makers of such vases was referred to by Aristophanes as a man "who paints the lekythoi for the dead." On vases of this sort the designs were painted over a chalky white slip, which covered the shoulder and most of

FIG. 6. Robinson Collection. Plate XXXI.

FIG. 7. Robinson Collection. Plate XLIV.

the body of the vase. On this slip figures were first drawn in outline and then details were added in color, so that the finished vase presented a highly polychrome appearance. A well preserved example in the Robinson collection (Fig. 7) is decorated with the commonest of all subjects in this class, the presentation of offerings at the tomb. The fundamental scheme is always the same. In the center of the field is a slender stele which marked the grave, usually crowned, as here, by a palmette, and on either side appear one or more standing figures, commonly with offerings in their hands. But in typical Greek fashion the details vary from one example to another and there are no exact duplicates. Other scenes, too, are frequently represented on these lekythoi: the dead lying in state, with mourning figures grouped around the bier; the body deposited at the tomb by the winged figures of Sleep and Death; the journey to the other world.

Finally, the plan of the Corpus calls for the publication not only of vases with painted or relief decoration, but also of more ordinary pots the importance of which is more archaeological than aesthetic.

Perhaps I ought to confess in closing that not all the works published in the *Corpus Vasorum Antiquorum* are as well preserved or as fine in quality as those that I have shown. I have naturally chosen among the best, but I hope that these few examples may suggest the varied interests which such a publication serves, and explain why it has been so loyally supported by the Council of Learned Societies.

GEOFFREY OF MONMOUTH'S MOTIVES FOR WRITING HIS *HISTORIA*

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(*Read, April 23, 1938*)

ABSTRACT

Geoffrey of Monmouth's *History of the Kings of Britain*, so vastly influential not only on subsequent literature, but also on historiography and even politics, was written between 1130 and 1138. Geoffrey undoubtedly was of British descent, probably Breton, since he exalts the Bretons and slurs the Welsh. One of his motives in writing was to provide the British people with an honorable early history, which they had lacked up to that time, but which their neighbors, the English and French of various provinces, were acquiring on a large scale just before he wrote. His other motives in writing were probably to afford precedent for the Empress Matilda's candidacy for the English throne; and also for the imperialistic ambitions of the Norman reigning house.

IF any single European work has been more influential over a long stretch of time than the early twelfth century Geoffrey of Monmouth's *History of the Kings of Britain*, I do not know what it is; influential, I mean, not only on literature throughout western Europe, but also on historiography and even on politics. It was quoted as affording early historical precedent for domination by England over Scotland and Ireland; the Scots resented it, and as lately as the seventeenth century the Irish could scarcely keep their temper when they spoke of it. It afforded the account of the early history of the island which was generally received at least as late as John Milton's *History of Britain*. As to its literary importance, it contains the earliest account of King Lear and his three daughters, which through various intermediaries bred what some have called the greatest achievement of the Teuton in poetry. Not to mention also Shakespere's Cymbeline, Milton's and Swinburne's Sabrina, and many another descendent, nearly a quarter of the work is devoted to

King Arthur, to the earliest known account of him except a list of his battles and certain relations with Welsh saints, mostly unflattering. This magnifying account, with vast accretions, has passed the king and his men down through Chrétien de Troyes, Malory, Spenser, Tennyson, Swinburne, to Wagner and E. A. Robinson, to say no more of other arts than the literary.

Geoffrey was of British parentage undoubtedly, as is shown by his manifest sympathies throughout, as well as by much other evidence. That he originated not from the Welsh but from some of the numerous Continental Bretons who followed the Norman conquerors to England is probably shown, besides much other evidence once more, by the fact that when he distinguishes between the two branches of his race he invariably disparages the Welsh and exalts the Bretons. His book is not extremely long, some two or three hundred pages, written in very good Latin prose of the medieval variety; not highly rhetorical, never long-winded, extremely well massed and constructed, alternating speed between rapid summary and more picturesque episodes, like a stream with rapids and quiet pools; not aiming at romance, nor at making fools gape, nor at pleasing pious zealots, but sober throughout, matter-of-fact, lifelike as the middle ages understood the lifelike. His models were the less credulous of historians familiar in his day. An eminent medievalist once conjectured that Geoffrey was deliberately writing a romance. Nothing of the sort; he was aiming solely at producing an account of events chiefly political in Britain from the fall of Troy to the seventh Christian century, such as would be accepted by intelligent and informed people who had no reason to doubt it. The only reason why at first some such people did doubt it is the facts about his sources. Aside from reading in not extremely numerous other writers, his chief known sources are Bede, Gildas and the so-called Nennius; but from even these he drew relatively little, least of all his main outline. The source he announces is a "very ancient book in the British language brought him from

Brittany." For a good many reasons I believe this book never existed, and was invented by Geoffrey to make his own work plausible. Such an invention has many parallels in both literary and historical writing, and should not be judged as severely back in the twelfth century as it would be in the twentieth. We today draw our most fundamental distinction between things true and false; the medievals drew theirs between things good and bad. Geoffrey had no doubt that in writing his *History* he was doing a good thing. He was a very able, intelligent, shrewd, sober, perceptive man; and out of what history was known, out of a very small amount of British tradition, and a very large amount of his own matter-of-fact invention grounded on events and conditions in his own day produced a book to convince those who lacked our conception of the historic process. In religion, politics, warfare, law, social conditions, geography, everything, the tissue that he wrought reflects England of the early twelfth century. Or such is my opinion.

Now why did he think this a good thing to do? The first reason is racial patriotism. There is no better measure of a people's civilization, as Gaston Paris once said, than its interest in its own history. The writing of national histories was a feature of the rapid advance of culture in the first third of the twelfth century, in contrast even with the late eleventh. In these years the Normans began to acquire their national history by Ordericus Vitalis (believed composed 1123–41), a good deal of it composed in the 1120's and possibly known then.[1] Anjou had the supposed Odo's *Gesta Consulum Andegavensium* (to 1107), and the short *Historia Andegavensis* probably by Count Fulk Réchin (died 1109). Flanders had among other things canon Lambert's *Genealogia Comitum Flandriae*, from 792 to 1120, very brief but history and not mere genealogy. The French had Hugh of Fleury's *Historia Francorum*, composed between 1109 and some time

[1] Admirably edited by A. le Prévost (Soc. de l'Hist. de France, 1838–55); see I, xlviii f. (by Delisle).—Much of this present article is undocumented assertion. The ample evidence, and much more on this same subject, will appear later in a far more extensive work on the legendary history of Britain.

after 1114.[1] The English, especially well-off, had the continuing *Anglo-Saxon Chronicle*, Florence of Worcester (to 1117), Henry of Huntingdon (first edition 1130), and best of all, William of Malmesbury's *Gesta Regum* and *Gesta Pontificum* (both 1125). I pass over sundry biographies, and local, recent or universal histories, and a few national histories written earlier. The above-named were written within thirty years before Geoffrey of Monmouth's *Historia* (1130–38). Almost all these accounts are interesting and readable, and some are distinguished and absorbing. They provide histories of England, and of France, Anjou, Flanders and Normandy, the highly civilized lands nearest and most important to England. All of these countries could thus nourish their pride on historical accounts extending over several or many centuries; England and France also on certain earlier ones of equal merit. To say that western peoples in the early twelfth century had a keen and fresh interest in their own early history is to speak moderately.

It is of no small significance to note to whom these works were dedicated. Hugh of Fleury dedicated his history to Adela of Blois, daughter of William the Conqueror and mother of Stephen, later king; and to Empress Matilda, later Stephen's rival. Henry of Huntingdon dedicated his *Historia* to Alexander, bishop of Lincoln, and William of Malmesbury his *Gesta Regum* to Robert, earl of Gloucester. All the others lack dedications, being mostly short, except that Ordericus Vitalis commemorates two abbots of his own convent. Several of these personages had not only acknowledged the finished books with favors (no doubt), but had even urged their writing. The above names are every one of them familiar to students of Geoffrey. To bishop Alexander he dedicated the Prophecies of Merlin in book VII of the *Historia*, and to Stephen (son of Adela) and to Robert of Gloucester the whole work. Matilda was warmly supported by Robert, and we shall see reason to believe, to the extent of his powers,

[1] Respectively, in *Chroniques des Comtes d'Anjou* (Soc. de l'Hist. de France, 1856–71); *ibid.*, pp. 375–83; *Monum. Germaniae Historica*, Scriptores, IX, 309–13; *ibid.*, 376–95. See also A. Molinier, *Sources*, II, 74–7, 308–9.

also by Geoffrey himself. Therefore precisely that circle, that higher circle, which Geoffrey looked up to had shown conspicuous interest in national histories, and had bestowed patronage on works which gave historical note to England and France. Geoffrey cannot have failed to be keenly aware of the activity in historical writing among peoples alien to him in the years shortly before he himself began.

But his own people, the Celtic British, were in no such happy case. Those of Brittany had nothing; the Welsh were little better off. The *Annales Cambriae*, a few pages merely, stopped then at 954, or perhaps, with a shorter continuation, at 1097; and what is worse are in no true sense a history, merely a few disjointed words to certain years on events in the British Isles and indeed the world which struck a Welsh monk as important. They had the *Historia Britonum* partly by Nennius, and the *Liber Querolus* on the fall of Britain by Gildas, constantly found together and referred to jointly by Geoffrey and others as Gildas. The former, from Adam to the eighth century A.D., while highly interesting in spots, is so confused as to be in the large unintelligible and offers no connected British history even till the eighth century, merely episodes, some of them fantastic. Gildas' narrative, only from the Roman invasion to the sixth century, is short, by no means lucid, and anything but gratifying to racial pride; the impressive part of the work is homiletic pessimistic scolding. "It was no part of the purpose of Gildas," says the greatest of modern Welsh historians, "to write a history of Britain."

The lack of accounts of British history was notorious among historians of this time. Geoffrey himself in his dedication voices his constant surprise that he finds so little in historians about Arthur and many other kings of Britain both before and after the Incarnation. The Brabantine monk Sigebert of Gembloux about 1111 finished one of the best universal chronicles in the middle ages. At its beginning [1] he promises to tell of the origin of the Romans,

[1] *Monum. Germaniae Historica*, Scriptores, VI, 300–1.

Persians, Franks, Vandals, English, Lombards, Visigoths, Ostrogoths, Huns; this he mostly does, usually going back to antiquity. It is obvious that here he wholly ignores the Britons. When he comes to the English he mentions the Britons, but merely their subjection to Rome, their conversion, their weakness against the Picts, and invitation to the Angles, who conquered the weak Britons and imposed their own law and name on them. Sigebert uses Bede, is unaware of the *Annales*, Nennius and Gildas, and shows other ignorance. As to historical importance and dignity the Britons cut a poor figure in Sigebert. And worse yet nearer home, in the pages of the great William of Malmesbury. Speaking of the monastic and literary activity of the abbey of Glastonbury, he alleges that it had been for years the abode of Gildas,—"neither an insipid nor a dull historical writer" is his possibly patronizing comment; and to this he adds worse—"to which Gildas the Britons owe it if they have any note at all among other peoples."[1] Hard luck to owe all their repute to Gildas even including Nennius!

Whether William's gibe at the Welsh is due merely to the facts, or to his own mixed descent from Normans and English at a time when the Welsh were resisting both, or to his lifelong residence in the southwest near the routes to south Wales, or to his admired Earl Robert's rule in Glamorgan, is irrelevant; but not so the extraordinary reference to William in the final words of Geoffrey of Monmouth's *Historia*. Having brought the British kingdom to a sorrowful end, dropped contemptuous remarks about the Welsh, and praised even the Saxons at their expense, he leaves the history of the later Welsh kings to Caradoc of Llancarfan,[2] and those of the Saxons to William of Malmesbury and Henry of Huntingdon; both of whom he bids be silent as to the kings of the Britons, since they do not possess that British book brought from Brittany by Walter of Oxford which he himself

[1] *De Antiquitate Glastoniensis Ecclesiae*, in Migne, *Patrol. Lat.*, CLXXIX, 1687; repeated in later versions of William's *Gesta Regum* (Rolls Ser.), p. 24.

[2] The facts about this author, hitherto so hazy, have been set forth by the present writer in *Speculum*, XIII (1938), 139–52.

has translated. Neither William nor Henry had shown any disposition to expatiate on the kings of the Britons—quite the contrary—and the personalness and intimacy of Geoffrey's remark is almost unparalleled in medieval historians. Some have thought it merely a tongue-in-the-cheek reference to his own purely fictitious source. But it certainly suggests some sort of personal relation between the men, which might be anything between chaffing and hostility. It is a retort. Geoffrey may have felt something which could be called jealousy toward William and Henry, *protégés* of Robert of Gloucester or Alexander of Lincoln. But it is hard to doubt that there was also racial feeling, in view of William's repeated sneer at the historical dusk in which the British peoples sat. A man like Geoffrey, of strong loyalty to the isolated British race, which lived much in its own past and its genealogies, a man himself, however, much in with more civilized races and a man of high cultivation, could not but view this historical dusk with the eyes of the civilized.

And could not but be moved to lighten it. This in very truth he does most handsomely. The *Historia* professes to give faithfully a far more ancient and complete history from the fall of Troy to the seventh Christian century than any other western people had except the Italians. The book put the British peoples not only in the running, but in its forefront. Geoffrey says in effect, like the celebrated wives of the juvenile crusaders, "We too have not been idle." This almost unquestionable motive for writing confirms one's disbelief in the existence of the "very ancient British book" alleged by Geoffrey. That, just at a time when neighboring peoples were acquiring creditable racial histories by the half-dozen, this for the Britons should turn up across the sea and leave no other trace strains one's credulity.

The only other motive to be mentioned here is what I shall crudely call political propaganda. It is quite evident that Geoffrey's social station in England was central rather than peripheral; that he had connections, which he valued, with the ruling race, the ruling caste and the ruling house.

He shows little settled disfavor to any people (except the Welsh), and certainly none to the Normans. (I disregard the Prophecies of Merlin in book VII, being very far from certain that he composed all of them.) There are two matters of political propaganda to be mentioned. One is dynastic, and regards the interests of the Empress Matilda. It will be remembered that 1 Jan., 1127,[1] Henry I declared her the heir to his throne, and then and several times thereafter made his magnates swear to accept her. Though her ambitions were defeated in 1135 by Stephen's promptness, and in 1141 by her own tactlessness, during all these years she had a strong prospect of reigning as queen and *Domina* of England and Normandy, and what is equally to the point her greatest supporter was Geoffrey's chief patron, the strong and generous Robert of Gloucester. This dynastic step of Henry I was entirely unparalleled in English or Norman or Welsh history; according to either history or tradition no woman had ever reigned.[2] Geoffrey yet more runs counter to precedent and expectation. In the first half and less of the *Historia* there are no less than four reigning queens—Guendoloena, Cordeilla, Marcia and Helena; and the father of Helena, like Henry, lacking other heir, has trained her to succeed to his throne. What is more, each of the four is distinguished in the diversified line of monarchs by goodness or capacity or both. A trusting reader of the *Historia* would find ample precedent for Matilda's reign and might well expect good from it.

The other piece of political propaganda I shall call, still more crudely, imperialist propaganda. That the Norman dynasty was addicted to extending its dominions needs no proof—extending by conquest or prudent marriages. Nor is proof needed that Geoffrey again and again exalts his Britons by showing them again and again as European if not world conquerors. I shall confine myself here to Arthur, who is a

[1] J. H. Round, *Geoffrey de Mandeville* (London, 1892), p. 31.

[2] One may think of Boadicea (Boudicca), who after the death of her tribal-king husband took the lead for a year or two. But she was unknown to early British and English writers, for Tacitus, the only Latin authority on her, was substantially unknown in the middle ages.

very Napoleon in his conquests and prudent marriages for his relatives. That Geoffrey had departed heroes in mind when depicting Arthur is certain, possibly Alexander and Cæsar, probably Charlemagne, perhaps more especially William the Conqueror (who like Arthur had had designs on Ireland) and also Cnut the Great. But I do not believe Geoffrey was unaware that his showing the ruler of England as lord also of all the British Isles, all Scandinavia and most of France, and as victorious over the Roman empire, would be highly gratifying to the Norman dynasty and its supporters. The *Historia* as has been said was later much used as affording precedents for English rule of other countries, including Ireland. I have a high opinion of Geoffrey's shrewdness and judgment, and am ready to believe that what actually did happen he foresaw might happen. He was not merely exalting racial heroes long dead. It is notable that the Arthur who later develops on alien French soil is no longer mainly the conqueror but rather the innocuous head of a brilliant and romantic court. The pseudo-historical Arthur continued to be celebrated in England till the seventeenth century; the romances proper developed in France with a quite different set of motives. It is an irony of literary history that Geoffrey's *Historia*, written in so matter-of-fact a style by so shrewd and sober a man and with such practical motives, should have begotten such a brood of fantastic romance and have nursed such a progeny of uplifting poetry.

ETYMOLOGY AND INTERPRETATION

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(*Read April 23, 1938*)

ABSTRACT

We often hear it said, on the ground of a word's etymology (real or supposed), that that word "really means," or else "ought to mean," something different from its commonly understood meaning. Some fifteen hundred years ago, Hindu scholars of the school called Mīmāṃsā, whose business it was to interpret the ancient Vedic texts, laid down a clear distinction between interpretation based on analysis or etymology (*yoga*), and interpretation based on conventionally established usage (*rūḍhi*). They taught that "rūḍhi is always stronger than yoga"; that is, one must always interpret a word in accordance with its conventional meaning (we should add, "in the appropriate period and dialect"), when that can be determined. Only when we don't know how the word is or was actually used (that is, chiefly in dealing with obscure historic records), should we depend on etymology for interpretation; and then only with clear realization that we are leaning on a weak staff. This Hindu doctrine is not only sound, but valuable for our day. It is recognized by most linguistic scientists, but those who lack training in that science often err by ignoring it. Even some linguists have not always kept it in mind.

AN Indologist finds it hard to be patient when confronted with the standard cliché about the "impractical and illogical Hindu mind." The popular notion seems to be that while the Hindus may be gifted "spiritually," whatever that means, they are incapable of close, careful reasoning, and tend to lose themselves in clouds of mystic vagueness, with little relation to common sense and hard facts. Such generalizations about whole peoples are probably never very sound, and while I think this one is peculiarly baseless, I do not wish to counter it with an opposite generalization which might also be hard to prove. Trying to be moderate, therefore, I shall venture to say that it needs only a little acquaintance with Indian cultural history to convince any openminded person that some Hindus have demonstrated scientific capacity of a high order. Perhaps "scientific capacity" is not the term I want: what I mean is that they have shown an ability to observe facts accurately, to reason closely and logically, and to keep

their feet on the solid ground of common sense and pragmatic usefulness. That may or may not be the same as scientific capacity, but it is substantially the opposite of the mental qualities attributed to Hindus generally by most people of my acquaintance.

Nowhere does this mentality show itself more clearly than in their dealings with language. Informed persons are well aware that Hindu achievements in descriptive grammar far surpass anything accomplished in that field by any other people on earth before the nineteenth century. In this paper I am going to call attention to one of their theories which belongs not to descriptive grammar but to semantics, and which was developed, quite characteristically, for an eminently practical purpose, namely, to help in interpreting obscure texts composed in a difficult and imperfectly known dialect, the dialect of the Veda. This theory has been largely neglected, even by Indologists. Yet it deserves attention. I think it is a sound working principle for interpreters of texts at any time or place, and that it could be studied with profit by more than one contemporary western scholar. Furthermore I believe it may help some of us to a sensible and realistic attitude towards our own native language.

It was first formulated by a Hindu school called the Mīmāṃsā, perhaps roughly about 500 A.D. (though possibly much earlier). This school was only one of a number which occupied themselves seriously with various aspects of language. The primary object of the Mīmāṃsins (as scholars in the Mīmāṃsā were called) was not linguistic at all. It was to draw up a sort of system of legal logic for interpreting the sacred books of the Veda, which they regarded as a kind of code of ritual laws, a set of ordinances for the performance of certain ritual acts. Considered from this standpoint (which is actually not so unrealistic as it might seem, in fact not a very serious distortion of what I think is the truth), the Veda appeared unsystematic and confused. The Mīmāṃsā sought to lay down principles of interpretation which would bring order out of the seeming chaos. Incidentally, in the course

of this endeavor, it became necessary to consider the verbal interpretation of Vedic texts, which were composed in an archaic dialect as remote from Classical Sanskrit as, perhaps, the language of Chaucer is from modern English.

Like all the general rules adopted by the Mīmāṃsā, the one under consideration is brought up in relation to a specific problem of interpretation. The Mīmāṃsins call it the "principle of the carpenter," *rathakāra-nyāya*. I shall ask your indulgence while I give you briefly the concrete situation, both because it helps to make the rule clear, and because it seems to me interesting, and characteristic of the very practical way in which this Hindu school worked. A Vedic text says: "A carpenter shall build the sacred fires in the rainy season." The word "carpenter," literally "cart-maker" (*ratha-kāra*), is commonly understood as the name of an occupational caste, workers in wood, holding a low rank in Hindu society and regarded as non-Aryan. As such, a "carpenter" cannot offer Vedic sacrifices, that being an exclusive privilege of Aryans. Why then should he be commanded to build the sacred fires at all, since they can be used for no other purpose than sacrifice? The Vedic statement doesn't seem to make sense. Here steps in the *pūrva-pakṣa*, the theoretical opponent or "devil's advocate," usually a man of straw set up only to be triumphantly demolished. Says he: The word *rathakāra*, "carpenter or cart-maker," need not be taken in its commonly accepted sense, but may be interpreted etymologically as meaning simply "one who makes carts." Since there is no reason why an Aryan should not make carts if he chooses, it refers in this passage simply to Aryans who have a possibly whimsical *penchant* for playing at carpentry or cart-making. For that matter, there would be no legal objection to an Aryan's making carts for sale, though custom would frown on it and it is doubtful whether it could be done for long.

This suggestion by the opponent is rejected by the Mīmāṃsā defender on the following ground. When we know the conventionally established usage (*rūḍhi*) of a word, we

must always interpret the word in that sense, never in accordance with what we take to be the word's etymology or derivation (*yoga*). "*Rūḍhi* always overrules *yoga*," is the principle. In this case it is inconvenient; it makes it very hard to get an intelligible interpretation out of the Vedic text. But that is no excuse for departing from sound philological principles.

One more example. There is a Vedic god Pūṣan. His name is supposed to be derived from a root *puṣ*, meaning "to prosper." In one passage, for reasons which I need not go into, it happens to be inconvenient to interpret the word Pūṣan as referring to this god. Again the devil's advocate, the *pūrvapakṣa*, suggests that we may take the word etymologically as meaning "the prosperer," and assume that it is not a proper name but merely an epithet of some other Vedic god; almost any Vedic god could be called "the prosperer."

No! says the Mīmāṃsā again. Pūṣan can only mean just simply Pūṣan, and there's an end of it. *Ruat caelum*, a word must mean just—what it means; just what the users of the language understand by it. No matter how much trouble it makes us, we dare not fly in the face of the principle that established usage, *rūḍhi*, overrules *yoga*, etymology or analysis, in interpretation. Only when the established usage is unknown or indeterminable, when we have no *rūḍhi* to guide us, is it allowable to resort to *yoga*, and then only *faute de mieux*, with full consciousness of the makeshift nature of this method. If for instance we didn't know the name of the god Pūṣan, and found the word in a text, we might guess that it had something to do with the root meaning "prosper," and assume tentatively that in the passage in question it might mean something like "he who prospers or causes prosperity." But this would be a mere guess; we should have no right to be satisfied with it, or to feel that we have really understood the word, until we have established its actual usage in real documents. And this not only, nor even chiefly, because sometimes our etymological theories may be wrong. Even when we may be justified in feeling quite certain that they are right, as in the case of the word *ratha-kāra* which is a trans-

parent compound exactly rendered in English by "cart-maker," we cannot presume to infer from this, *a priori*, the actual significance of the word as used in the living language for the simple reason that possible changes of meaning in the actual use of words are practically infinite in range, or at least quite unpredictable.

This last dictum seems to be a hard saying for many persons. Surely, I have been told again and again, and sometimes by very distinguished scholars—surely there are some words of such transparent formation, made with elements so simple, so standard, and so invariable in meaning, that structural analysis can, infallibly, give us the real meaning. So I have been told, and when I have asked for examples, I have received two suggestions, viz. diminutive suffixes, and negative prefixes. Let us briefly examine both. But first let me remind you that the claim is for infallibility. It must be so, to be relevant. Either by analyzing a word we can determine its meaning with one hundred per cent accuracy and certainty; or else, we need some other criterion, which can be only established usage. If *yoga* fails in a single instance, then in that instance at least (and who knows in how many others?) it is no safe guide; we must after all fall back on *rūḍhi*. For if all that is meant is that *sometimes* one who knows the components of a word can infer its meaning, that needs no argument; it is conceded in advance. It only means that sometimes the results of interpretation by *rūḍhi* and by *yoga* coincide. All I say, with the Mīmāṃsā, is that whenever they differ, *rūḍhi* alone must prevail; and that until usage is known, we cannot be *sure* of *any* word's interpretation. If analysis gives us a word's true meaning, that is a lucky accident. We *know* that it *is* the true meaning only on the ground of known usage.

Well, then, the German suffixes *-chen* and *-lein* have been suggested to me as so transparent and invariable in meaning that one can always say, at least with reasonably close approximation (let this pass for the sake of argument), what a formation containing them would have to mean, if one

knows the root-word to which they are affixed, without inquiring just how the language uses the derivative containing the suffix. I pass over the matter of the considerable variety of shades of meaning which, everybody knows, are often associated with diminutive formations: such varieties as the "simple" diminutive of smallness, the endearing, pitying, contemptuous, and so forth. Leave all that aside, and consider merely the one word *Fräulein*. Obviously it is formed by adding the diminutive suffix *-lein* to the word *Frau*, which we know means "woman" or "wife." Now take any possible shade of diminutive meaning, out of the list suggested rather than enumerated above, and by applying it to *Frau*, I submit you will never be able to infer the actual, present-day meaning of *Fräulein* in the German language. Perhaps the idea of "young," which has some vogue as a kind of diminutive value, would come the closest; it may indeed be true that once upon a time *Fräulein* meant a young *Frau*. Yet we all know that at present a *Fräulein* may be over eighty years old, and a *Frau* less than sixteen.

Or take any negative prefix, say the English *un-*. It is certainly about as transparent and about as standardized as any formative element in the language. Undoubtedly *un-able* means "not able," *un-clean* "not clean," and so with innumerable other cases. But remember that if we are to admit *yoga* as a final authority in interpretation, it must work one hundred per cent. Let me ask you, then, how analysis of the forms could bring you to any difference between the English words *unspeakable* and *unmentionable?* By etymology, both these words should mean, and no doubt they once did mean, simply "that cannot, or should not, be spoken of or mentioned." They should be synonyms, by *yoga;* I cannot see the slightest basis, *a priori*, in their formations, for making any distinction between them. But are they synonyms? Never mind running to the dictionary; just ask yourselves whether "an *unspeakable* thing" means to you the same as "an *unmentionable* thing." In this sophisticated age, the latter phrase generally evokes a smile; the former never would.

In the course of time, our language has, by pure convention, and with no regard for etymology, come to associate with these words wholly different connotations.

So much for the principle; now for its practical values for us. I think there are two of these. First, it should be borne in mind by all those who concern themselves with the interpretation of, especially, obscure and imperfectly known languages. With them the temptation to interpret by etymology is peculiarly strong, and many scholars even of the present day succumb to it. In some cases, indeed, they cannot be criticized for doing so, provided they realize what they are doing, and do not rate too highly the validity of their results. The Mīmāṃsā itself does not say that *yoga*, etymology, should never be used in interpretation, but only that it must always be overruled by *rūḍhi*, established convention, when that can be determined. Unfortunately, too many scholars tend to feel that when they have suggested a plausible etymology for a word, they have already implied a satisfactory interpretation of it. They should learn from the Mīmāṃsā that such an interpretation is by its very nature on a lower plane than an interpretation based on study of the actual usage of the word in documents. (I assume, of course, that they have no access to living documents, to speakers of the dialect they are studying.) In short, interpretation by etymological analysis can never, in the nature of things, result in anything but a guess as to the meaning. There is no objection to using it in a tentative way, precisely as a preliminary guess. Not infrequently it may help to put us on the right track; although it must be added also that it has very often proved a will-o'-the-wisp and led investigators away from the truth. The main point however is that nothing is ever settled about the meaning of a word until *rūḍhi* has reported: until we have found by experience just how the word was used in the language.

Most of us, however, are not too actively concerned in the interpretation of such languages as Hittite and Venetic. The second application of our principle will perhaps have

more universal interest, because it relates to the use of all languages, including our own mother-tongue.

We sometimes hear it said, or implied without explicit statement, that a word "really means" this or that, when what should be said is, either that it once had that meaning, or that its etymology is such and such. More than this, some people, even scholars, will recommend using words in senses which are, or which seem to them, consistent with etymology, and deplore what they call the "distorted" or "degenerate" uses which (unfortunately to their minds) have come to be accepted. Some have even gone so far as to deplore the use of a word at all without full conscious knowledge of its derivation. Archbishop Trench, in his celebrated monograph "On the Study of Words" (22d edition, New York, 1902, p. 51 f.), quotes with enthusiastic approval Coleridge's dictum that "in order to get the full sense of a word, we should first present to our minds the visual image that forms its primary meaning." Reflection on the Mīmāṃsā principle will suggest that the so-called "primary meaning" is quite irrelevant to the "full sense" of the word as actually used today. If you doubt this, try it out on one of the archbishop's own examples. I will try it for you. Take the word *desultory*. He says that, in using this word, we ought always to think of the Latin *desultor*, which means a circus-rider who was adept in the art of jumping rapidly from the back of one horse to another in full gallop. Well, this is an interesting etymology, I agree. Knowledge of it should certainly add to the spice of life for most intelligent people. But the plain fact is that we do not ordinarily think of it, most of us, even if we have heard of it, when we use the word *desultory;* and most people who use that word have probably never heard of the etymology. Here is the test: one who used that word in such a way that comprehension of his meaning would depend on knowledge of the etymology, would simply not be understood by most English-speaking people. In passing, let me mention another difficulty with the Coleridge-Trench position. How shall one define "primary" in

the phrase "primary meaning"? At what historic point would etymological justice be satisfied? There is nothing ultimate about the Classical Latin word *desultor*. It is derived from *desilire*, "to jump down or off." *Desilire* in turn has its own etymology; and so on. However far back you go, you will never get to a word that has no history, though of course that history will not always be known to us. Logically, there is no more reason for stopping at the Latin *desultor*, in adding overtones to the English *desultory*, than at any other point in the *regressus ad infinitum*.

Those who, like the good archbishop, deplore and try to change this condition have about as much chance of success as King Canute with the waves of the sea. This is perhaps a slight exaggeration. In an individual case or two, for special reasons, success may be conceivable. Language has been changed by the deliberate act of one man. The Flemish chemist Van Helmont in the seventeenth century invented a new word, *gas*, which has since been adopted, I suppose, by all or most civilized languages. If some contemporary Trench wants to induce his fellow-speakers of English to use certain words, or words in general, in what he calls their "primary" senses, whatever he chooses to mean by that term, let him, in the vernacular phrase, go to it. There is nothing immoral or anti-social in his ambition. I don't think he will succeed, unless in a few rare and special cases. But if he does succeed in an isolated case or two, what will he have done? He will have changed the meaning of a word or two, and so changed the language, just as truly as Van Helmont changed it when he got people to adopt his new word *gas*. It makes no difference in principle whether the change consists in resurrection of a dead past, or creation of an absolute novelty. And until he succeeds in this, it is unrealistic for him to talk about the "full sense" of the word, as now used in the language, as if it were or could be anything other than the sense understood by the general run of speakers of the language who use it.

At this point I find by experience that some one is apt to want to raise a crusading banner and tilt a lance against me in

defense of historical linguistics. The less he knows about that discipline, the more excited he is apt to get. Real linguistic scholars rarely have any quarrel with the Mīmāṃsā doctrine I have set forth. If at first they question it, I generally find that this is due to imperfect understanding of it, doubtless because I have failed to present it to them clearly. Let me assure the tender-hearted that I myself find etymological science extremely interesting, and have even devoted a good part of my professional life to it. Not only is it a fascinating study in itself, but it has important uses for other disciplines. It is no derogation of it to warn against misuse of it. On the contrary, it should tend to rehabilitate it in the eyes of the judicious, to relieve it of a certain odium which the naivetés of Trench and his successors have to some extent, and in some quarters, laid upon it. In general, as I have indicated, most modern scholars of good linguistic training act as if they accepted the Mīmāṃsā doctrine, even though they may never have heard it so formulated. I could, to be sure, quote examples of what seem to me to be violations of it, from men whose linguistic training and accomplishments make one expect better things from them. Even some linguists, therefore, could perhaps profit by reflection upon the proposition that *rūḍhi* always overrules *yoga*. But usually it is people with little or no linguistic training who talk unrealistically about this as well as other aspects of language. Since everyone uses language, unfortunately almost everyone thinks he knows what it is. These remarks may be considered in one sense a plea for the necessity of studying historical linguistics, in order, among other purposes, to know how to avoid the misuse of it.

TRIPLOIDY IN THE NEWT, TRITURUS VIRIDESCENS

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(Introduced by Edwin G. Conklin)

(*Read April 21, 1938*)

ABSTRACT

In various species of amphibians, triploid embryos and larvæ have been found occasionally in normal, preserved material. The repeated occurrence of such cases indicated that it should be possible to discover triploid individuals among living larvæ and to follow their development.

The tailtips of one hundred larvæ of *Triturus viridescens* were amputated and stained *in toto*, and the chromosomes were counted in mitotic figures in the epidermis of the tailfin. Ninety-six of the larvæ showed the diploid number of chromosomes, twenty-two, while the remaining four were triploid (thirty-three chromosomes). In the triploid tailtips, the nuclei of epidermis cells, lateral line organs, and red blood cells are on the average larger than in diploid controls. The nuclei of molt layers cast off during metamorphosis also tend to be larger in the triploids. It is thus possible to collect conclusive evidence of triploidy while the animals are developing normally.

The general course and the rate of development of the triploids were normal. In three of the triploids, the body size was only slightly increased over that of the controls, while the fourth showed definite signs of gigantism from an early larval stage on. The four triploids were preserved during or at the close of metamorphosis.

Some of the general problems that are raised by the natural occurrence of triploidy in a class of vertebrates are under investigation at the present time, viz., the frequency of triploidy in nature, the origin of the triploid embryos, the general effects of polyploidy, and the influence of polyploidy on sex determination.

INTRODUCTION

THE frequent occurrence of polyploidy in plants contrasts sharply with the relative scarcity of polyploid animals. According to Müntzing ('36), over one-half of all flowering plants may be regarded as members of polyploid series, since their chromosome numbers are simple multiples of basic numbers that are found in related species or varieties. A review of all known cases of polyploidy among animals has recently been published by Vandel ('37). In a few species of invertebrates, mostly arthropods, triploid or tetraploid races occur in nature. Among vertebrates, the only known instances of polyploidy

are the triploid embryos of frogs and salamanders which have been found in preserved material by several investigators (Table 1). With the exception of one parthenogenetic individual, these triploids were discovered among normal, supposedly diploid embryos which served as controls in various kinds of experiments.

TABLE 1

TRIPLOID AMPHIBIAN EMBRYOS FOUND IN PRESERVED MATERIAL

Author	Species	Number of Triploid Embryos or Larvæ Found
G. and P. Hertwig ('20)	*Rana esculenta*	18 (all from same female)
Dalcq ('30)	*Rana fusca*, *Rana esculenta*	2 out of 43
Parmenter ('33)	*Rana pipiens*	1 parthenogenetic larva
Fankhauser ('34)	*Triton palmatus*	2 out of 14
Fankhauser and Kaylor ('35)	*Triturus viridescens*	2 out of 11

The number of embryos investigated with regard to their chromosome number is too small to permit any conclusion as to the frequency with which triploid individuals appear in various species of amphibians. Still, the fact that they have been found in relatively small groups of embryos indicates that they are not too rare. It should therefore be possible to discover triploid individuals among living larvæ and to follow their development. Because of the high percentage of triploid embryos found in two species of newts, larvæ of *Triturus viridescens* were selected for this investigation.

The present paper deals with the methods employed in the identification of triploid larvæ, particularly with the criteria of triploidy that are available in a study of living material. This will be followed by a brief account of the development of triploid animals, and by a short discussion of the general problems that are involved in the investigation of polyploidy in vertebrates. A preliminary note on the most important results has already been published (Fankhauser, '38a).

THE IDENTIFICATION OF TRIPLOIDS AMONG LIVING LARVÆ

In October 1937, a large number of female *Triturus viridescens* were caught in a pond near the Fox Gap, Pennsyl-

vania. During November and December, egg-laying was induced by means of implants of anterior lobes of pituitary of *Rana pipiens* (see Kaylor, '37).

When the larvæ had reached a stage with three or four finger buds, the posterior one-third of the tail was amputated and fixed in Bouin's fluid. The tailtips were stained in toto in Harris' acid hæmalum. Mitotic figures were always present in the epidermis of the tailfin. In 100 larvæ, obtained from five different females, one or more figures were clear enough for an accurate count of the chromosomes. Ninety-six of these tailtips were found to be diploid, with 22 chromosomes in all analyzable mitoses. The numbers that were counted in the remaining four tailtips are shown in Fig. 1:

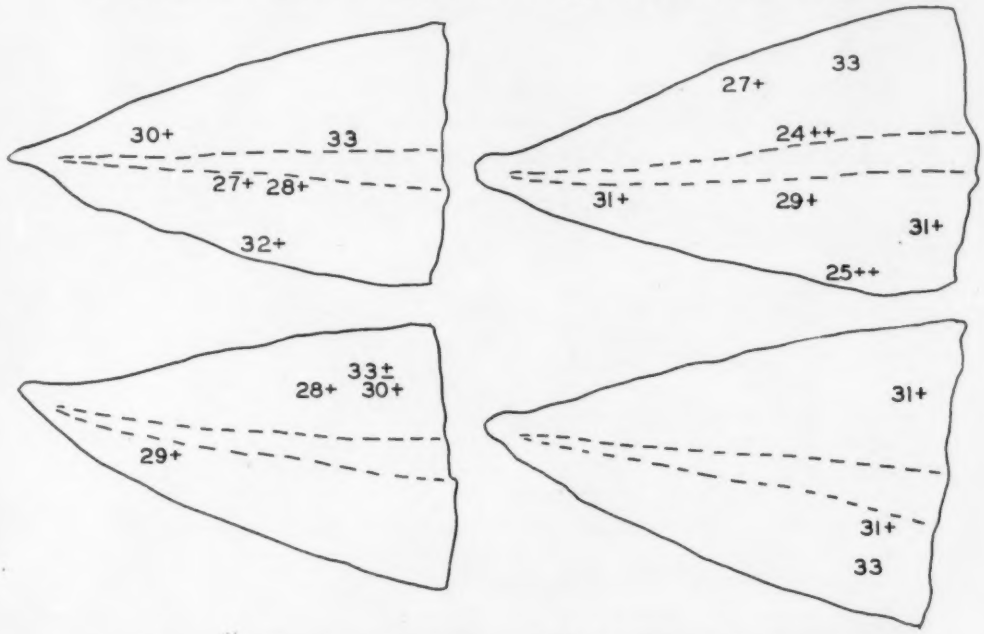

FIG. 1. Outline drawings of the amputated tailtips of the four triploid larvæ, showing the location of the mitotic figures in the epidermis in which the chromosomes could be counted more or less accurately. "30 +" indicates that at least 30 chromosomes were present, probably more.

the triploid number, 33, could be counted accurately in at least one cell in each tailtip; other figures, which were not

sufficiently clear for an accurate enumeration, contained from over 27 to over 32 chromosomes. It is therefore very probable that all these cells are actually triploid.

In photomicrographs of mitotic figures, the chromosomes cannot be counted accurately because of the size and crowding of the individual elements. Two of the clearest triploid metaphase groups are shown in Figs. 8 and 9, Pl. 1. In early anaphase, the chromosomes are still more crowded (Figs. 10 and 11), but an approximate count is sometimes possible under the microscope.

While photographs do not allow an exact determination of chromosome numbers, they are very valuable in demonstrating differences in the general aspect of triploid and diploid figures (Figs. 12 to 25). In early and late prophase, the triploid nuclei are larger. In metaphase and anaphase, the triploid chromosome plates are larger and appear more crowded. In telophase, the triploid daughter nuclei are again larger than the diploid (Figs. 24 and 25).

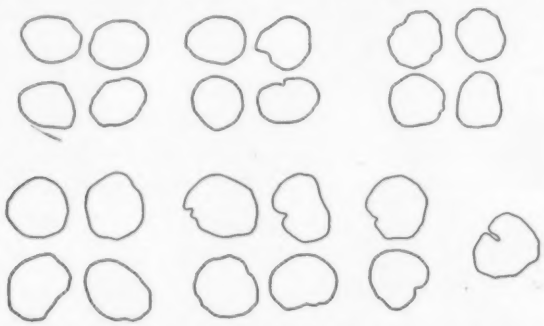

FIG. 2. Drawings of nuclei in prophase from the epidermis of the tailfin. Upper row: from three diploid control larvæ (Nos. 1, 3, and 4). Lower row: from the four triploid larvæ (Nos. 5, 10, 11, and 20). × 243.

All the epidermis nuclei in prophase which were present in the four triploid tailtips were drawn at the same magnification as the corresponding nuclei from three controls (Fig. 2). The triploid nuclei are, without exception, strikingly larger.

This size difference between triploid and diploid nuclei in the same stage of mitosis indicates that we may apply here,

as in many other cases, a secondary criterion of triploidy, namely, a general increase in the average size of the interkinetic nuclei of various tissues. This is shown clearly by the nuclei in three different types of cells:

(1) The nuclei in the epidermis of the tailfin (Fig. 3), in spite of their irregular shape and of great individual varia-

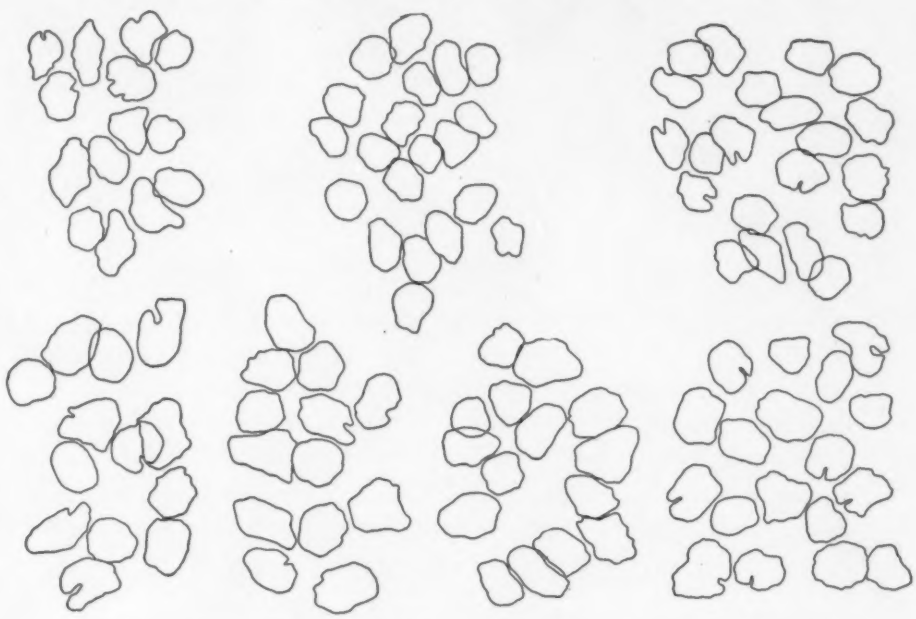

FIG. 3. Drawings of groups of nuclei from the epidermis of the tailfin. Upper row: from three diploid control larvæ (Nos. 6, 12, and 16). Lower row: from the four triploid larvæ (Nos. 5, 10, 11, and 20). × 210.

tion, are on the average definitely larger in the four triploid tailtips than in the controls.

(2) On either side of the axis of the tail, a row of rudiments of lateral line organs is present at this stage. The nuclei in each rudiment vary considerably in size, but the average size is again clearly larger in the triploid tailtips (Fig. 4). The greater nuclear and cell size may lead to an increase in the size of the rudiment as a whole, if the number of cells remains about the same as in diploid organs.

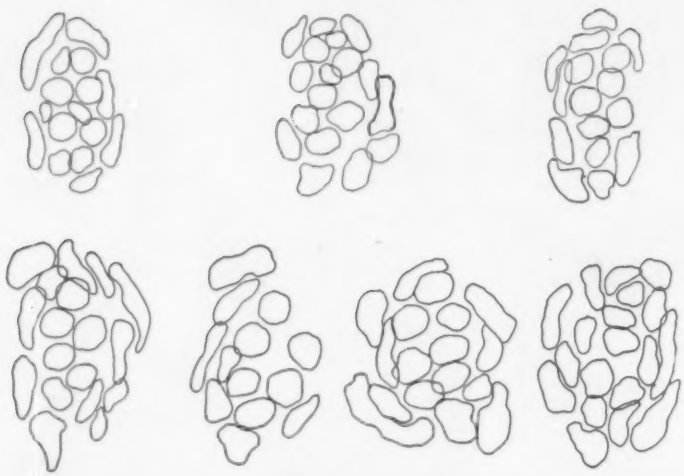

FIG. 4. Drawings of the nuclei of rudiments of lateral line organs near axis of tailtip (compare Fig. 6). Upper row: from three diploid control larvæ (Nos. 6, 12, and 17). Lower row: from the four triploid larvæ. × 365.

(3) At the time of amputation of the tailtips, the first capillaries appear in the tailfin. Occasionally, a few red blood cells are present. In three of the triploid tailtips, the nuclei of several erythrocytes could be drawn and compared with those from several controls (Fig. 5). They are strikingly larger in two of the triploids, and slightly larger in the third.

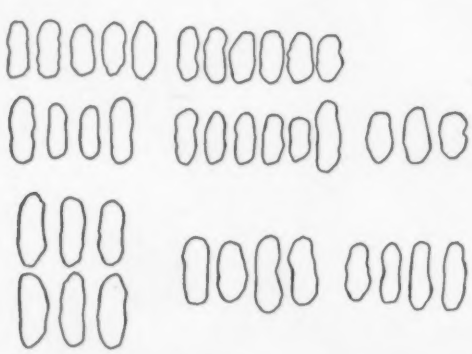

FIG. 5. Drawings of nuclei of red blood cells, from capillaries in the tailfin. Above: five groups of nuclei from five diploid control larvæ. Below: nuclei from triploid larvæ, six from No. 5, four from No. 10, and four from No. 20. × 478.

Both the lateral line organs and the red blood cells do not originate from tailbud material, but from primordia located in two other regions of the embryo (Fig. 6). The tailtip thus includes samples of cells from at least three widely separated parts of the embryo, and is therefore truly representative of the nuclear condition of the embryo as a whole. This is important in view of the possible criticism that the tail alone, and not the whole larva, might be triploid. The formation of a limited triploid area in an otherwise diploid embryo, moreover, would be much more difficult to explain

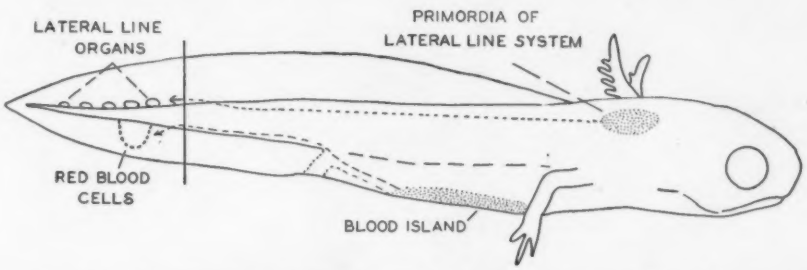

FIG. 6. Diagrammatical drawing of a salamander larva to show the origin of the rudiments of the lateral line organs and of the red blood cells which are present in the tailtip. The amputated tailtip includes cells from widely separated regions of the embryo.

than the origin of a completely triploid individual, which has been definitely traced, in several cases in both plants and animals, to the union of an unreduced, diploid egg with a normal, haploid sperm. This, of course, does not imply that every cell of a triploid individual contains exactly 3n chromosomes, since occasional irregularities in the distribution of chromosomes, which occur even in diploid embryos, may be expected more frequently in the presence of an additional set of chromosomes, for purely mechanical reasons (see Wettstein, '27).

At the beginning of metamorphosis, another opportunity presents itself for a test of the nuclear condition of the animals. The outermost layer of the whole epidermis is shed repeatedly. Pieces of these molt layers may be fixed, stained, and the

nuclei compared as to their relative size and spacing. Fig. 7 illustrates the differences which may be observed between triploid and diploid molt layers; the triploid nuclei, on the average, are larger and somewhat more widely spaced, as

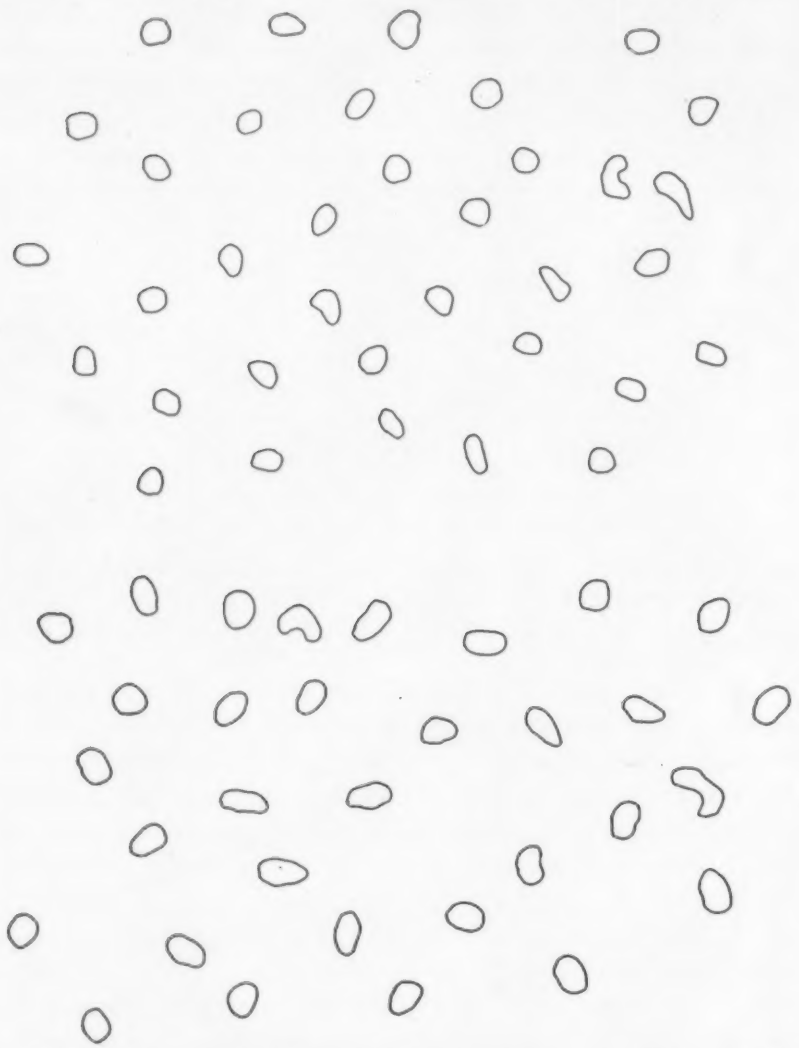

FIG. 7. Groups of nuclei from the first molt layer of a diploid animal (No. 6, above) and of a triploid (No. 5, below). The nuclei of the triploid are, on the average, larger and somewhat more widely spaced. × 320.

would be expected if the cells, as well as the nuclei, have a larger size.

As many pieces of shed skin as possible were collected from different animals, as well as from several subsequent molts of the same animal. The results were not entirely consistent, primarily since we are dealing here with dead or dying cells in which the nuclei may represent different stages of condensation and disintegration.

Considered as a whole, the evidences of triploidy gathered while the animals develop normally are entirely conclusive: direct evidence from chromosome counts in tailtips, supplemented by indirect evidence from the comparison of the size of nuclei in various types of cells in the tailtips, and, to a certain extent, in pieces of molt layers cast off during metamorphosis.

The Development and External Appearance of Triploid Larvæ

Among plants, single polyploid individuals may often be discovered by their general appearance. Triploid Œnotheras, for instance, may be recognized by large and heavy foliage, by large buds and flowers, and by the small amount of pollen produced (Capinpin, '33). This "semi-gigas" type, originally described in triploid evening primroses by Stomps and Lutz, is produced by the larger size of the individual triploid cells.

Triploid animals may also be larger than diploid individuals of the same species, *e.g.*, in the isopode, *Trichoniscus provisorius* (Vandel). In Drosophila, triploid females are larger according to Morgan, Bridges, and Sturtevant ('25, Fig. 11), while Dobzhansky ('37, p. 223) finds that they differ in their habitus but do not show a noticeable increase in body size.

Three of the four triploid larvæ of *Triturus viridescens* were only slightly larger than the controls (Pls. 2 and 3). This difference in size was not noticed until the triploid condition had been established by chromosome counts. Dur-

ing metamorphosis it became less pronounced and disappeared almost entirely (Figs. 29 and 33). The three animals were preserved before or after the completion of metamorphosis.

The fourth larva (No. 20) was, from an early larval stage on, considerably larger than the controls and was suspected to be triploid before chromosome counts were made (Fig. 34). At this stage the melanophores are more clearly distinguishable individually than later on, when they show more ramification and begin to form a dense network with the neighboring pigment cells. They appear larger in the triploid larva; however, this may be due in part to a greater degree of "expansion" of the cells, since in the control they appear, on the average, more condensed. As development proceeded, the difference between the triploid and the controls was maintained or became even more pronounced (Figs. 35 to 37). The triploid died five days after the beginning of metamorphosis. It was still considerably larger than a diploid animal in the same stage of metamorphosis (Fig. 38), or any other diploid animal at the beginning or end of metamorphosis.

The general course of development, as well as its rate, was entirely normal in the four triploids, as far as the records show. This does not preclude the possibility that more careful observations on a larger number of triploid larvæ may disclose significant deviations from the normal. However this may be, the general effects of triploidy seem to be less obvious than in many plants, where triploid individuals are not only larger, but also show a reduced growth rate (compare Müntzing, '36).

Discussion

A number of important questions are raised by the natural occurrence of triploidy in amphibians. Since these problems are under investigation at the present time or will be studied in the near future, they will be discussed but briefly in the following paragraphs. A detailed treatment of the general problems of polyploidy will be found in several recent publications (Wettstein, '27, Artom, '28, Heilborn, '34, Lindstrom, '36, Müntzing, '36, Darlington, '37, Dobzhansky, '37, Vandel, '37).

(1) The Frequency of Triploidy in Nature

Since the tailtip method may be generally applied to amphibian larvæ, it will now be possible to determine more accurately the frequency with which triploid individuals appear in different species. For *Triturus viridescens*, the observations reported in this paper would put the percentage of triploid larvæ at 4 per cent. This figure is certainly too high, as was shown by the examination of 211 tailtips in the course of the past few months, among which a single triploid tailtip was discovered (Griffiths, unpublished data). For the two series together, the frequency would thus be about 1.6 per cent.

More important than the determination of the general frequency is the fact that triploidy does not appear purely at random: the four triploids described in this paper all developed from eggs deposited by the same female. This does not prove that the extra set of chromosomes was derived from the mother, since, during one laying period, the eggs are all inseminated by spermatozoa from the same male which are stored in the spermatheca after mating. It shows, however, that certain matings have a greater tendency to produce triploids than others. For this particular family the percentage of triploids is as high as 9.5 per cent (4 larvæ out of 42).

In Œnothera cultures, triploidy has long been known to occur more frequently in some families than in others, reaching a maximum value of 12 per cent in certain strains (Shull, '29). It is possible that, in Triturus also, an inherited tendency to produce triploid offspring is present in certain lines. It may bring about this result by preventing synapsis, as in the case of Mendelian asynapsis in maize, or by interfering in some other way with normal meiosis.

(2) The Origin of Triploid Embryos

In plants, triploid individuals appear to arise usually from the union of an unreduced, diploid egg with a haploid sperm. The reciprocal combination, fertilization of a haploid egg by a diploid sperm, or by two haploid sperms (dispermy), occurs

rarely. One case has recently been reported in maize (Rhoades, '36).

The triploid embryos of *Rana esculenta* obtained by G. and P. Hertwig ('20) also originated from diploid eggs, because eggs of the same female, when activated to gynogenetic development by spermatozoa of *Bufo viridis*, produced diploid "false hybrids." The occurrence of diploid eggs in frogs has also been demonstrated cytologically by Bataillon and Tchou-Su ('29) and by Tchou-Su ('36).

Diploid eggs may originate in various ways: (1) from tetraploid ovarian tissues by normal maturation, (2) from diploid oocytes by omission of the reduction division, and (3) from diploid oocytes and normal meiosis, followed by retention of the chromosomes of the second polar body. Bataillon's observations suggest that the second process is involved in the production of diploid eggs in the frog.

In both plants and animals, the rate of production of diploid gametes may be increased by artificial means, particularly by exposure to low temperatures. Rostand ('34, '36) claims that a treatment of toad's eggs, immediately after laying, with temperatures close to freezing for several hours, may prevent normal maturation and produce a considerable number of diploid eggs. I have not mentioned before that, together with other eggs of *Triturus viridescens*, two of those which developed into triploid larvæ had been exposed to such a treatment. At present, it seems rather doubtful that this should have been the actual cause of triploidy. If it had been involved, we would possess a very convenient means of controlling the production of triploid animals. The influence of low temperature will therefore be studied in detail.

(3) *General Effects of Triploidy*

In many plants, triploidy produces a moderate degree of gigantism, intermediate between the normal condition and the gigantism of tetraploids. In some polyploid plants, however, all "gigas" or "semi-gigas" characteristics are completely absent. These tetraploids or triploids do not differ markedly

from diploid individuals of the same species. This exceptional reaction has led to a considerable amount of speculation as regards the causes that are involved. Darlington ('37) suggested that it might be due to the segregation of dwarfing factors, preventing possibly harmful consequences of gigantism. Some organisms may be more strictly adapted to a specific size so that polyploidy would have a deleterious effect. This condition may be the cause of the "failure of polyploidy to appear in many groups of flowering plants," and "is perhaps in part responsible for the general absence of tetraploidy amongst animals."

Wettstein ('27) and Heilborn ('34) have called attention to the fact that animals, more so than plants, often exhibit a high degree of constancy of cell numbers in the various organs ("eutely"). Polyploidy, causing an increase in nuclear and cell size, "is bound to lead to a heavy increase in organ size and often to abnormal development. This may account for a more or less complete extermination of polyploids among animals (where eutely is strong)" (Heilborn, '34).

The almost complete absence of gigantism in three of the four triploid Triturus larvæ strongly suggests that polyploid animals may be perfectly able to compensate for an increase in cell size, and to keep the size of various organs close to normal. This is brought about by a decrease in cell number, as a preliminary study of the serial sections through the four triploids has shown. The comparison of the microscopical anatomy of these animals with that of normal diploids and of the metamorphosed haploid Triton tæmiatus (Baltzer, '22), which has been described in two recent papers (Fankhauser, '38 b, c), promises valuable contributions to our knowledge of the factors regulating cell numbers and of the general problem of cell constancy, which has hardly been touched in vertebrate animals.

(4) *Effects of Triploidy on Sex*

In bisexual animals with a definite sex chromosome mechanism, the presence of a third set of chromosomes usually

disturbs the normal process of sex determination. While the homogametic sex (female in Drosophila, male in moths) is normal, the heterogametic sex (male in Drosophila and Carausius, female in moths) is changed to an intersexual condition. However, certain exceptional cases have been noted: in the isopode, *Trichoniscus elisabethæ*, for instance, both triploid males and females are normal (Vandel, '34, '37).

We have at present no conclusive cytological information concerning the sex chromosome mechanism in amphibians, although there are indications that the male sex is heterogametic. In this case the effects of both haploidy and triploidy on sex may be similar to those first described in Drosophila (compare Fankhauser, '37, '38c). In *Triturus viridescens*, sex differentiation takes place shortly before metamorphosis. It is possible that a close study of the condition of the gonads of the four triploid animals will contribute to the solution of this problem.

(5) *Possible Occurrence of Tetraploidy in Amphibians, and of Polyploidy in Higher Vertebrates*

If diploid spermatozoa and eggs are produced but occasionally, the chances are very slight that two exceptional gametes will meet and produce a tetraploid zygote. Thus a tetraploid larva may not be discovered unless a very large number of tailtips are examined. Tetraploidy by somatic doubling (in the first cleavage division) might also occur. So far, no evidence of the occurrence of tetraploid larvae in nature has been found.

Since triploidy is fairly frequent among amphibia, it is not unreasonable to assume that triploid individuals may also occur among reptiles, birds, and mammals. Unfortunately, chromosome studies in higher vertebrates often meet with considerable difficulties, notably a small cell size and, frequently, a large number of chromosomes. In various species, the normal, diploid chromosome number has not yet been established with certainty. It will not be an easy task, therefore, to demonstrate either the existence or the absence

of triploidy, or of higher forms of polyploidy, in these groups of vertebrates.

Summary

1. The tailtips of one hundred larvæ of *Triturus viridescens* were amputated and stained in toto, and the chromosomes were counted in mitotic figures in the epidermis of the tailfin.

2. Ninety-six of the larvæ showed the diploid number of chromosomes, twenty-two, while the remaining four were triploid (thirty-three chromosomes).

In the triploid tailtips, the nuclei of epidermis cells, lateral line organs, and red blood cells are on the average larger than in diploid controls. The nuclei of molt layers cast off during metamorphosis also tend to be larger in the triploids.

It is therefore possible to collect conclusive evidence of triploidy while the animals are developing normally.

3. The general course and the rate of development of the triploids were normal. In three of the triploids, the body size was only slightly increased over that of the controls, while the fourth showed definite signs of gigantism from an early larval stage on. The four triploids were preserved during or at the close of metamorphosis.

4. The general problems that are raised by the natural occurrence of triploidy in a class of vertebrates are discussed briefly, viz., the frequency of triploidy in nature, the origin of the triploid embryos, the general effects of polyploidy, and the influence of triploidy on sex determination.

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Explanation of Plate I

Figs. 8 to 25. Photomicrographs of mitotic figures in the epidermis of the tailfin, from whole mounts of the tailtips, stained with Harris' acid hæmalum. × 418.

Figs. 8 and 9. Two particularly clear metaphase figures from the triploid larvæ Nos. 20 and 10, in which exactly 33 chromosomes could be counted under the microscope.

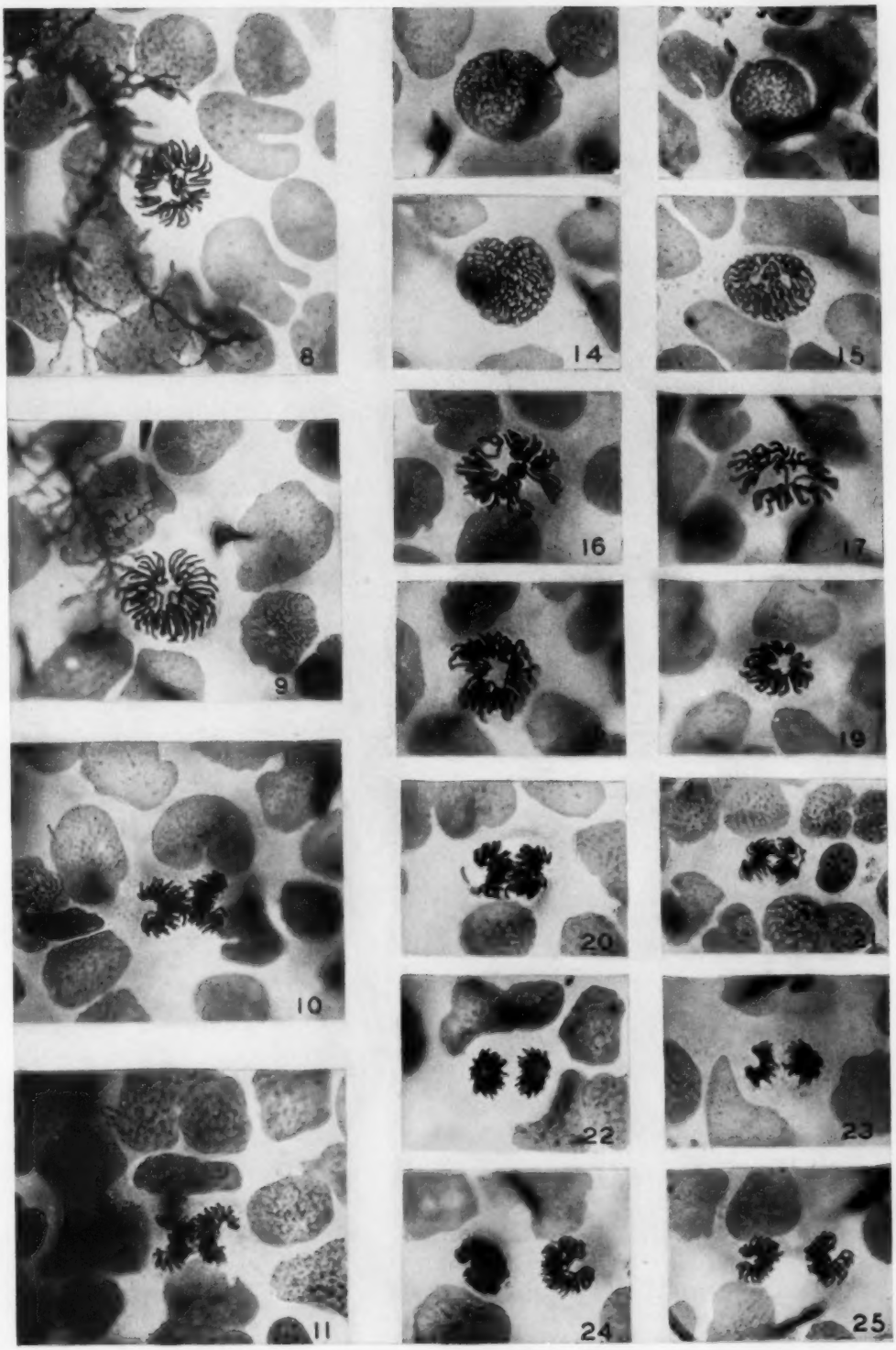
Figs. 10 and 11. Two early anaphase figures from the triploids Nos. 20 and 11. The chromosomes are too crowded to allow an exact enumeration, but more than 22 are certainly present.

Figs. 12 to 25. Corresponding stages in triploid and diploid mitosis. Triploid figures on left (even numbers), diploid figures on right (odd numbers).

12 and 13 Early prophase.
14 and 15 More advanced prophase.
16 and 17 Early metaphase.
18 and 19 Later metaphase.
20 and 21 Early anaphase.
22 and 23 Late anaphase.
24 and 25 Telophase.

In pro- and telophase, the triploid nuclei are clearly larger than the diploid. In meta- and anaphase, the chromosome plates are larger and the chromosomes more crowded.

PLATE I



Explanation of Plate II

Figs. 26 to 29. Photographs of four stages in the development of a triploid animal (No. 5) and a diploid control (No. 6). Triploid on left.

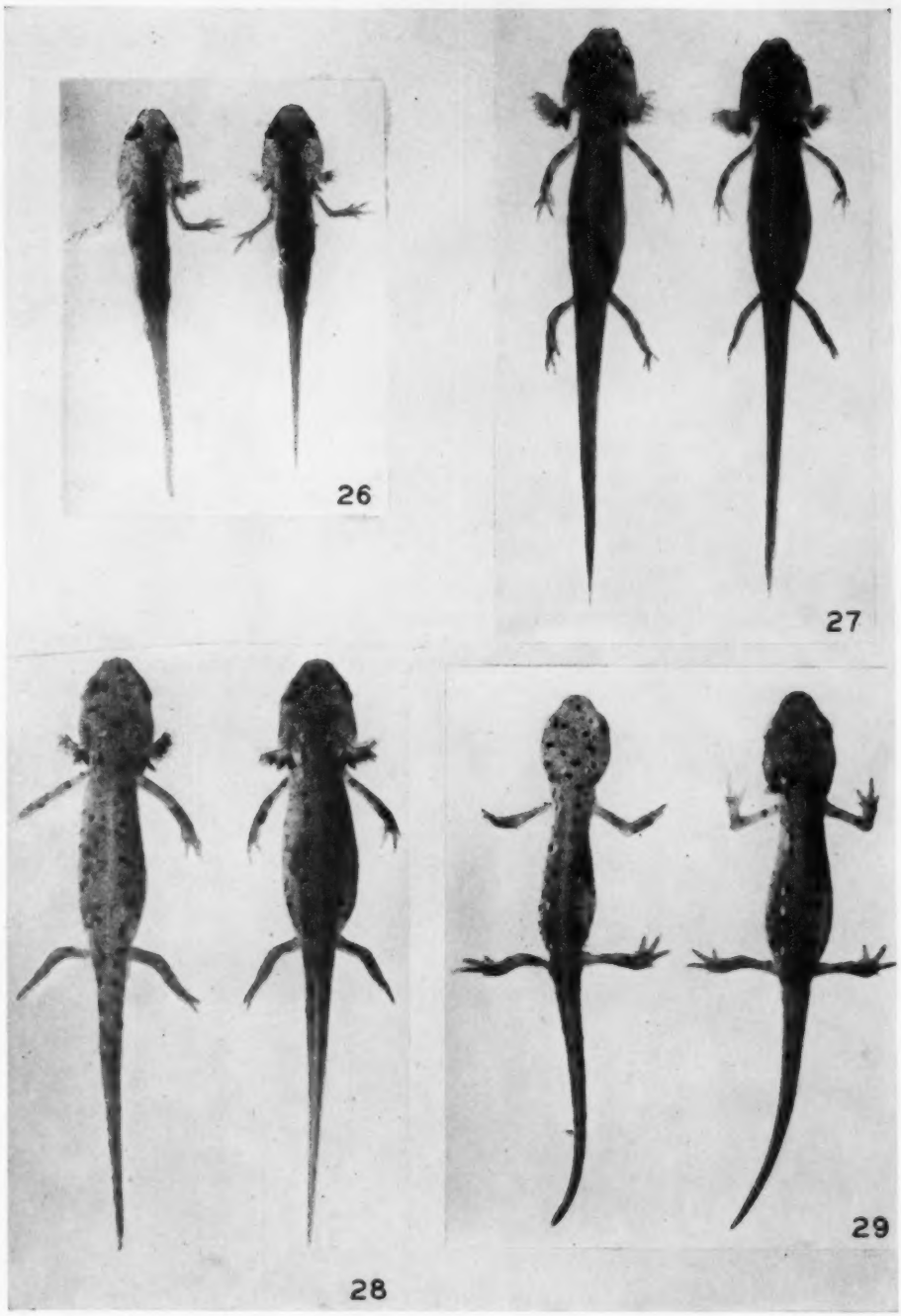
Fig. 26, 52 days. The triploid appears slightly larger. × 3.6.

Fig. 27, 86 days. Triploid still slightly larger, pigment pattern begins to change to adult condition. × 2.7.

Fig. 28, 95 days. The triploid has shed the first molt layer (beginning of metamorphosis), pigment pattern more pronounced (black spots on orange-brown background). In the control, the first molt did not take place until the 102nd day. × 2.7.

Fig. 29, 116 days. Triploid animal at end of metamorphosis, diploid in earlier stage of this process (note difference in shape of head and presence of short gill rudiments in diploid). The lightly colored rudiments of the vermillion spots of the adult are visible in both animals as white areas. The triploid is hardly larger than the control. × 2.7.

PLATE II



EXPLANATION OF PLATE III

FIGS. 30 TO 33. Four stages in the development of another triploid animal (No. 10) and a diploid control (No. 13 in Figs. 30 to 32, No. 3 in Fig. 33). Triploid on left.

FIG. 30, 49 days. × 3.6.

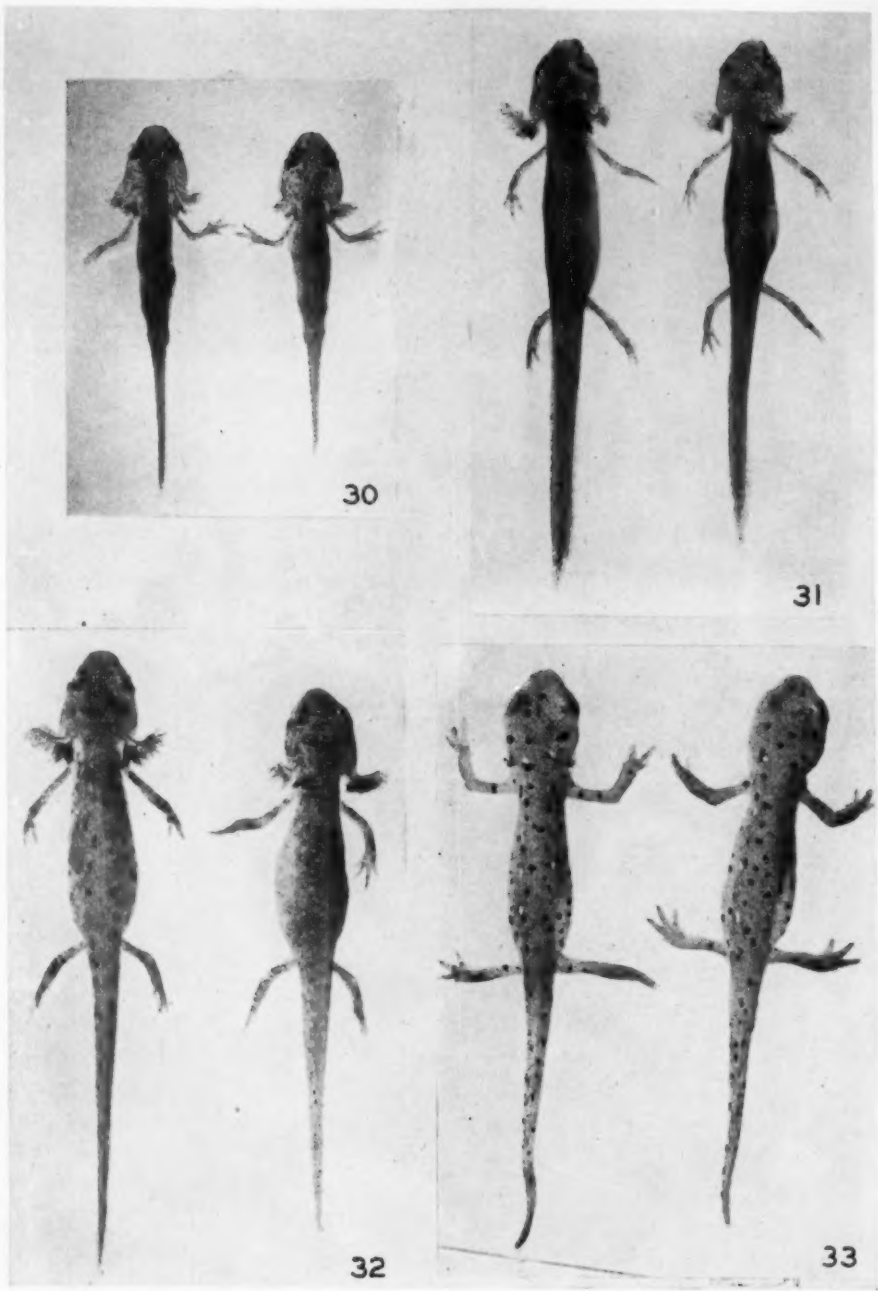
FIG. 31, 82 days. × 2.7.

FIG. 32, 91 days shortly before metamorphosis. × 2.7.

During larval development, the triploid remained slightly larger. Metamorphosis began on the 96th day in the control, on the 102nd day in the triploid.

FIG. 33, 125 days. Triploid still with short gill rudiments. It is shown together with another control that began metamorphosis at the same time. × 2.7.

PLATE III



Explanation of Plate IV

Figs. 34 to 38. Five stages in the development of the triploid larva No. 20 and of a diploid control (No. 16 in Figs. 34 to 37, No. 2 in Fig. 38). Triploid on left.

This triploid was considerably larger than the controls from an early larval stage on.

Fig. 34, 39 days. The melanophores on the head appear larger in the triploid. × 3.6.

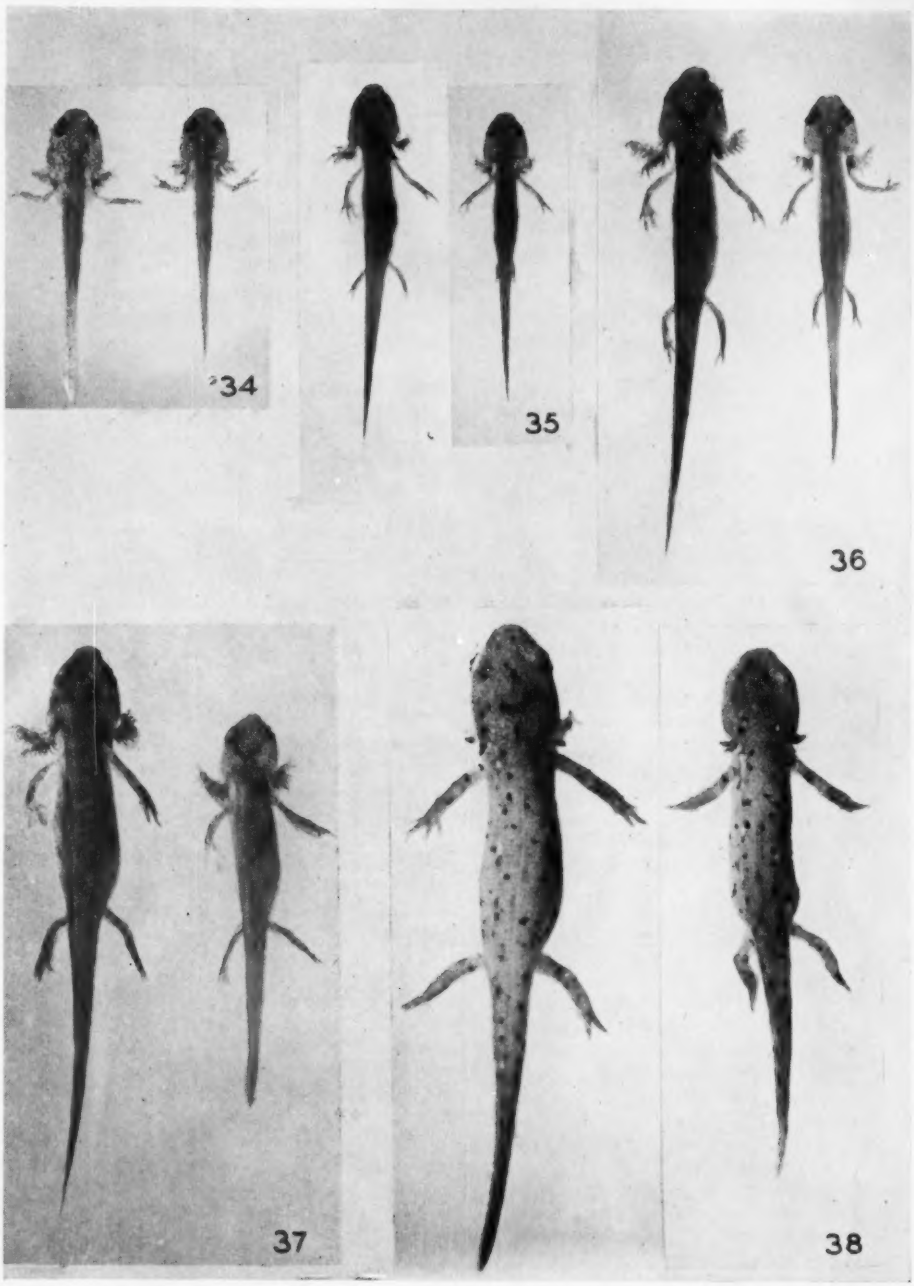
Fig. 35, 64 days. × 2.2.

Fig. 36, 72 days. × 2.7.

Fig. 37, 81 days. × 2.7.

Fig. 38, 109 days. The triploid died in this stage (five days after beginning of metamorphosis). The diploid control, No. 2, was photographed in about the same stage of metamorphosis. × 2.7.

PLATE IV



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A B C D E F G H

114° 118° 122° 126°

10° 14° 18° 22° 26° 30°

1 2 3 4 5 6 7 8 9 10

Joseph Bonapa

Gulf

Admiralty Gulf

Montague Sound

York Sound

Brunswick Bay

Prince Regent R.

King Sound

Collier Bay

Carnot Bay

Napier Broome Bay

BAGU

BEMBA

MANDE

WULU

WIRNGI

WEMBRIA

YEIDJI

ARNGA

UNAMBAL

WUSARA

KENYOON

ANDERJA

UNGARINYIN

UNGAMI

BARDI

NJAUI

NYUL-NYUL

DJABERA-DJABER

NGORNBAL

NYGINA

DJUKAN

YAUOR

KARADJERI

MANGALA

WOLMERI

BUNABO

KUNAN

WALKI

KEHA

MUNMULLA

BOONAR

Roebuck Bay

Port Smith

La Grange Bay

Eighty Mile Beach

NANGAMADA

PEDONG

WIDAGARI

BALGU

IBARGA

NGALUMA

KARIERA

NGARLA

NYAMAL

WEEDOOKARRY

MADOJUNJUNERA

INJIBANDI

NOALA

BAILGU

PANJIMA

BINIGURA

Northwest Cape

Chabjuwardoo Bay

TALAINJI

BAJONG

BURDUNA

JIWALI

TENNA

CHURORO

TARGUDI

NGADARI

WAIAN-WONGA

WIRDINYA

INA-WONGA

NGALA-WONGA

WARIENGA

WAJERRI

TARGARI

NALA

INGARDA

INPIRRA

Naturaliste Channel

Shark Bay

Geographe Channel

Gantheaume Bay

NGADAWONGA

NGAJA-WONGA

WONGMULLA

MALGANA

MULIARRA

YABAROO

CHEANGWA

WATCHANDI

NANDA

EAW

TARDARICK

NATINGERO

YINDI

DITU-WONGA

NGAJU-WONGA

BARDU-WONGA

YIRA-WONGA

NANGA-WONGA

KUPUDU-WONGA

Lake Carnegie

Lake Gregory

Lake Stanley

Lake Disappointment

Lake Lefroy

Salt Lakes

Sandhill

Hampton Range

Robinson Range

Hunter Range

Fitzroy R.

Ord R.

Margaret R.

Dora

L. Blanche

L. Auld

L. Gillan

L. Wells

L. Way

L. Mairland

L. Darlot

L. Throssell

L. Carey

L. Ballard

L. Seabrook

L. Deborah

L. Yinderlgooda

L. Lapage

L. Breaden

Gregory Salt Bos

L. Isabella

Lady Edith's

L. Gwli

L. Tobin

MINAL-YUNGA

KATABUNGATA

MOORO

H
I
J
K
L
M
N
O
130°
134°
136°
Joseph Bonaparte Gulf
Gulf of Carpentaria
Cape Wessel
Cape Arnhem
Caledon Bay
Groote Eylandt
Port Roper
Limmen Bight
Wellesley Is.
Port McArthur
Melville I.
Bathurst I.
Clarence Str.
Cockburn Sound
Gordon Bay
Port Patterson
Darwin
Shoal Bay
Port Darwin
Beagle Bay
Torres Bay
Sturt Bay
Buccaneer Bay
Blue Mud Bay
Tiwi
Kakadu
Umoriu
Gunwinggal
Minnitji
Abedal
Grimbio
Malalli
Rainbarngo
Boun
Barlamomo
Murngin
Yaernungo
Djinba
Yandjiung
Nawara
Gunmitji
Wadjiung
Yunggor
Madngella
Maranunggot
Berringin
Djerait
Mullik
Ponga
Okamod
Kungadakan
Wilwanga
Bergurduruk
Marithiel
Nangor
Murinbata
Norgan
Bagu
Bemba
Wulu
Mende
Wiringi
Yeidji
Membria
Agma
Anderdal
Mumandil
Yamandil
Djerag
Keha
Munmulla
Kunan
Walki
Boonara
Lunga
Djaru
Kisha
Nining
Munga
Mariu
Tookurra
Mirung
Mando
Tewinbol
Bulinada
Kwaranjee
Binggingina
Walpari
Warramunga
Tjingilli
Kooringee
Laytha
Kudenji
Umbaia
Karawa
Goonanderry
Anula
Laierdila
Wallogo
Wilingura
Illawarrie
Gnanji
Gnuin
Yangkala
Yukulu
Wanyi
Nicholson River
Injilinji
Ngoodoorindi
Mygoodan
Ngarra
Mingin
Mikoolun
Koonkurri
Miiubi
Wallenwatta
Workoboongoo
Kalkadoon
Workia
Hitakudi
Kaitish
Wulmala
Ilpira
Unmatjera
Iliaura
Yarding
Yanindo
Yunnaletnka
Undekerebina
Waloo-kera
Rungo
Pitta-pitta
Yunda
Wonkajera
Bojnji
Ulaolinyi
Yuntalintaya
Koonkoolenya
Rukkia
Miorli
Ngulubulu
Karanya
Tunderri
Pintubi
Pitala
Mangini
Lake Amadeus
Luritja
Aduntа
Waiangara
Winpanda
Maiulatara
Pitjintara
Ullparidja
Jankunjadjara
Wonkamala
Salt Lakes
Yelkuyendi
Wonkongaru
Wonkatyeri
Karanguru
Yauarawaka
Marula
Mandjindja
Ngalia
Alinjerra
Andigarina
Wycanga
Ngameni
Ngurawola
Yaurorka
Yantruwunta
Kaliwiruru
Wongai
Willara
Urabunna
Dieri
Madutara
Tangara
Kogara
Boundary Dam
Mulatara
Minal-yungar
Katabungata
Mooroon
Wanbiri
Yirgla-meening
Tidni
Tula
Gebbera
Kuyani
Wailpi
Yadliaura
Wadikali
Mardala
Dilaarda
Ngurunta
Malya-mara
Endawarra
Wilyakali
Tongaranie
Wilyali
Wirangu
Head of Bight
Lake Eyre
Salt Lakes
L. Macdonald
L. Mackay
L. Gregory
L. Blanche
L. Hart
L. Torrens
L. Frome
L. Eyasord
L. Harris
L. Labyrinth
L. Cadibarrawirracanna
L. Acraman
L. Macfarlane
Island Lagoon
Milgunn
Yarcla
Wyola
Nullin
Ngandi
Ngalbon
Yandaran
Mara
Yikil
Angadigie
Wallaba
Wallura
Tyngman
Mungarra
Waddaman
Ngainman
Mudburra
Gnanji
Gnuin
Binbinga
Wadere
Gallawa
Yabuyu
Inkura
Djulirri
Djulian
Wakya
Ngandi
Ngalbon
Yandaran
Mara
Yikil
Angadigie
Nullin
Wallaba
Tyngman
Mungarra
Waddaman
Ngainman
Mudburra
Gnanji
Gnuin
Binbinga
Wadere
Gallawa
Yabuyu
Inkura
Djulirri
Djulian
Wakya

P
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U
145°
10°
2
14°
3
4
18°
5
6
22°
7
8
26°
9
10
30°
Strait
PRINCE OF WALES I.
Newcastle Bay
Orford Bay
Shelburne Bay
Temple Bay
Weymouth Bay
Lloyd Bay
Cape Direction
Port Stewart
Princess Charlotte Bay
Cape Melville
Cape Flattery
Cape Bedford
Weary Bay
Cape Tribulation
Cape Grafton
Trinity Bay
Mitchell R.
Palmer R.
Russell R.
Eliott R.
Flinders R.
Staaten R.
Gilbert R.
Leichhardt R.
Norman R.
Barron R.
Mulgrave R.
Hann R.
Morgan R.
Johnstone R.
Tully R.
Herbert R.
Murray R.
Hull R.
Lucinda Point
Rockingham Bay
Hinchinbrook I.
Halifax Bay
Bowling Green Bay
Port Denison
Edgecumbe Bay
Repulse Bay
Whitsunday I.
Broad Sound
Shoalwater Bay
Port Clinton
Keppel Bay
Curtis I.
Sandy Cape
Fraser I.
Hervey Bay
Wide Bay
Moreton Bay
Stradbroke I.
Point Danger
Western R.
Warrego R.
Paroo R.
Barcoo R.
Thomson R.
Diamantina R.
Georgina R.
Wilson R.
Macumba R.
Neales R.
Finke R.
Todd R.
Hay R.
Alice R.
Burke R.
Darling R.
Barwon R.
Condamine R.
Nogoa R.
Comet R.
Dawson R.
Mackenzie R.
Isaac R.
Belyando R.
Suttor R.
Lynd R.
Mitchell R.
Gregory R.
Nicholson R.
Leichhardt R.
Flinders R.
Cloncurry R.
Georgina R.
Diamantina R.
Barcoo R.
Thomson R.
Warrego R.
Paroo R.
Darling R.
Murray R.
Murrumbidgee R.
Lachlan R.
Macquarie R.
Castlereagh R.
Bogan R.
Namoi R.
Gwydir R.
Barwon R.
Condamine R.
Balonne R.
Maranoa

INGARDA
INPIRRA
MALGANA
WATCHANDI
NANDA
Gantheaume Bay
8
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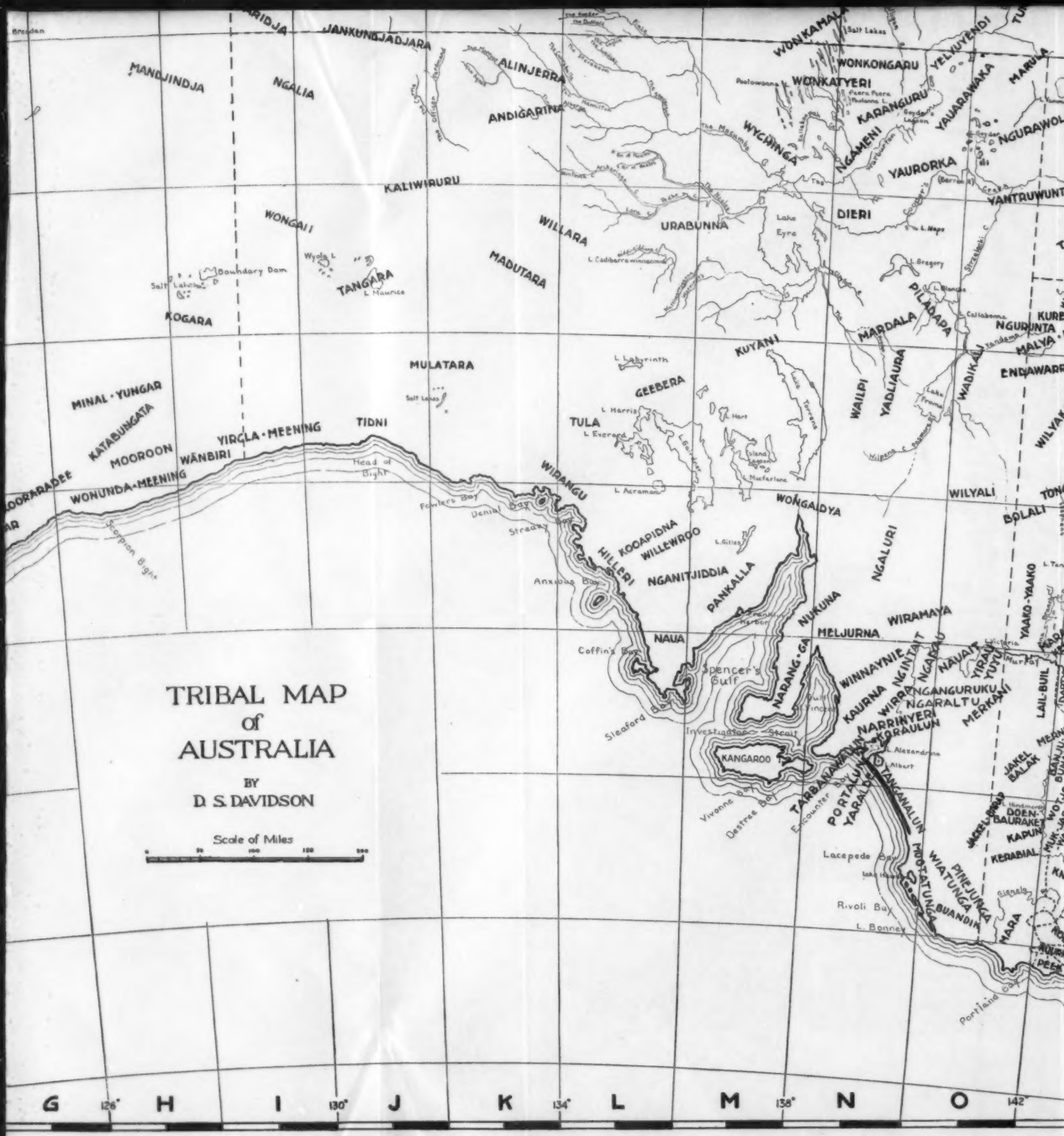
25°
30°
34°
110°

YIRA-WONGA
KUDU-WONGA
NANGA-WONGA
NGADA-WONGA
NGAJA-WONGA
NGAJU-WONGA
BARDU-WONGA
WONGMULLA
MULIARRA
YABAROO
CHEANGWA
TARDARICK
DITU-WONGA
YINDI
NATINGERO
MINAL-Y
KOORARADEE
KARKAR
WONUND
MONG-UP
WHEELMAN
CARACTERR-UP
QUAL-UP
YUNGA
BALLARDONG
WHAJOOK
YOBBERORE
WEEL
KIKKAR
NGOKGURRING
KUNYUNG
KOJONUP
MEERAMAN
MEENUNG
KARRANGUP
PROPELMAN
KOKAR
MINUNG
WARANGGO
MBANANGER
Geographic Bay
Flinders Bay
Brookes Isle
Ratcliffe Bay
Tor Bay
Chapman Bay
Bedford Harbor
Ann Haven
Culham Inlet
Careful Bay
Sunday Bight
Lake Carnegie
L. Gillen
L. Wells
L. Throssell
L. Way
L. Maitland
L. Darlot
Salt Lakes
L. Carey
L. Ballard
L. Bitter
L. Lepage
L. Yinderlgoonda
L. Deborah
Seabrook
Lake Lefroy
Lake Gawan
The Johnston Lakes
L. Dundas
L. O'Connor
L. Hurlstone
L. Varley
L. Pen
L. Camm
L. King
L. Hope
Yerra Yerra Lakes
Swan R.
Avon R.
Murray R.
Williams R.
Blackwood R.
Preston R.
Collie R.
Donnelly R.
Capel R.
Ludlow R.
Gordon R.
Kent R.
Deep R.
Frankland R.
Kalgan R.
Gairdner R.
Arthur R.
Oldfield R.
Stanford R.
Ewlyamartup R.
Yeo L.
Rason L.
A
B
C
D
E
F
G
114°
118°
122°
126°


TRIBAL MAP
of
AUSTRALIA
BY
D. S. DAVIDSON
Scale of Miles
JANKUNDJADJARA
MANDJINDJA
NGALIA
ALINJERRA
ANDIGARINA
WONKONGARU
WONKATYERI
KARANGURU
YELYUTYENDI
YAURORKA
NGURAWOL
KALIWIRURU
WONGAII
WILLARA
URABUNNA
DIERI
MADUTARA
TANGARA
KOGARA
MULATARA
MINAL-YUNGAR
KATABUNGATA
MOOROON
WANBIRI
YIRCLA-MEENING
TIDNI
WONUNDA-MEENING
GEEBERA
TULA
KUYANI
WAILPI
YADLIAURA
MARDALA
PILADAPA
NGURUNTA
MALYA
ENDAWARR
WILYALI
BOLALI
WIRANGU
KOOAPIDNA
WILLEWROO
HILLERI
NGANITJIDDIA
PANKALLA
WONGALDYA
NGALURI
NUKUNA
WIRAMAYA
MELJURNA
NAUA
NARANG-GA
WINNAYNIE
KAURNA
NGARALTU
MERKANI
NARRINYERI
KORAULUN
KANGAROO I.
TARBANANALUN
PORTALUNGA
YARALDI
TANGANALUN
PROTATUNGA
WIATUNGA
PINEJUNGA
BUANDIK
DOEN-BAURAKET
KERABIAL
KAPUNDA
JAKEL BALAK
LAIL-BUIL
YAARO-YAAKO
Spencer's Gulf
Investigator Strait
Encounter Bay
Rivoli Bay
Lacepede Bay
Portland Bay
Sleaford Bay
Coffin's Bay
Anxious Bay
Streaky Bay
Denial Bay
Fowlers Bay
Head of Bight
Scorpion Bight
Boundary Dam
Wyola L.
L. Maurice
L. Labyrinth
L. Harris
L. Everard
L. Hart
L. Gilles
L. Acraman
L. Torrens
L. Frome
L. Eyre
L. Bonney
L. Alexandrina
L. Albert
Lake Eyre
Lake Torrens
Island Lagoon
Salt Lakes
Wilpena
Collabonna
Strzelecki C.
Cooper's Creek
Diamantina R.
Warburton R.
Georgina R.
Finke R.
Todd R.
Macumba R.
Hamilton
Alberga
Neales R.
Peake
Denison
Mt. Dare
Dalhousie
Mt. Margaret
Mt. Squires
Mt. Wedge
Mt. Finke
Mt. Hamilton
Mt. Chandler
Mt. Sarah
Mt. Davenport
Mt. Eba
Mt. Serle
Mt. Lyndhurst
Mt. Arden
Mt. Gason
Mt. Freeling
Mt. Barry
Mt. Hopeless
Mt. Victoria
Mt. Remarkable
Mt. Lofty
Mt. Crawford
Mt. Pleasant
Mt. Barker
Mt. Gambier
Mt. Schank
Mt. Burr
Mt. Muirhead
Mt. Benson
Mt. Gunson
Mt. Eyre
Mt. Finniss
Mt. Hindmarsh
Mt. Brown
Mt. Hay
Mt. Dutton
Mt. Cooper
Mt. Bryan
Mt. Lyell
Mt. Sturt
Mt. Wood
Mt. Willoughby
Mt. Hope
Mt. Ive
Mt. Fitton
Mt. Christie
Mt. Brisbane
Mt. Arrowsmith
Mt. Hughes
Mt. Lindsay
Mt. Bonython
Mt. Cavenagh
Mt. Cockburn
Mt. Dennison
Mt. Dromedary
Mt. Eliza
Mt. Elizabeth
Mt. Fenton
Mt. Gardner
Mt. Gilruth
Mt. Hann
Mt. Hart
Mt. Hayward
Mt. Hill
Mt. Hillier
Mt. Hood
Mt. Howitt
Mt. Humphris
Mt. Jagged
Mt. Jeffcott
Mt. Kitchener
Mt. Lawson
Mt. Leichhardt
Mt. Liebig
Mt. Lindsay
Mt. Lloyd
Mt. Lucas
Mt. Macdonnell
Mt. Magnificent
Mt. Mann
Mt. Margaret
Mt. Mary
Mt. Mitchell
Mt. Moffatt
Mt. Morgan
Mt. Murchison
Mt. Murray
Mt. Newman
Mt. Olga
Mt. Palmer
Mt. Peake
Mt. Phillips
Mt. Pleasant
Mt. Ramsay
Mt. Renner
Mt. Riddock
Mt. Robertson
Mt. Russell
Mt. Sandiman
Mt. Scott
Mt. Shaw
Mt. Skinner
Mt. Stevenson
Mt. Stuart
Mt. Sturt
Mt. Tate
Mt. Thomas
Mt. Tietkens
Mt. Todd
Mt. Tomkinson
Mt. Tooth
Mt. Vansittart
Mt. Wedge
Mt. Wells
Mt. Whitfield
Mt. Wilkinson
Mt. Willoughby
Mt. Wilson
Mt. Woodroffe
Mt. Young
G
126°
H
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130°
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142

KURNANDA-BURI
ERO-MARRA
BUNTA-MURA
KURGOIN
PEECHERA
GNOREE
KUNGADITJI
THIBALLA
WONKA-MURRA
KUWATCHEE
BITHARRA
POIDGERRY
KALALI
BAD-JERI
PARUINDI
KONGAIT
MOMBA
BARUNGA
KURNU
BADRUMBINYA
WOMBUNGEE
NGEUMBA
WALWAN
WILYAKALI
PERNOWRIE
BUNGYARLEE
NAUALKO
MILPULKO
BAKANDI
TONGARANKA
TUNG-ARLEE
KARAURA
KARIN
BERRI-AIT
WONGAROH
WIRADJURI
TATI-TATI
ITA-ITA
MURRING-BULLA
KUTA-MUNDRA
NARRANDERA
MATI-MATI
WI-THAI-JA
BARABA
YARR
GUNDUNGURRA
NGUNAWAL
WOLGAL
KURIAL
BALUNG-KARAR
GINNING-MATONG
PINPANDOR
NGURA-LUNG
NATRAK
YIRUN
YA-ITMA-THANE
THEODORA
MOGULLUM
WARRING
BUTHERA
TAOUNGURONG
BRABRA-LUNG
BIDUELLI
BRAYAKU-LUNG
BUNURONG
TATUNGA-LUNG
BRATADA-LUNG
COLIGAN
KNINDO
JAJAU-RUNG
WUDTHAURUNG
JUPA-GALK
JURO
BURARA
NGARIGO
BEMERIN-GAL
PAIENDRA
KATUNGAL
GUYANG-GAL-YUIN
THAWA
KORENG WARRAK
WORKARA
YAWAI
NUKU-BURA
MUNDAN-BURA
EMON
TARRAMBUL
KUAM
KAMBUWUL
UNGGRI
MURRUNGAMA
KOGAI
UNGHI
KABI
KAIA-BARA
WAKA
PAIAMBA
KINGKI
HOOLOOLA
TURRBAL
WOGEE
NOONU-KUL
GOEN-BUL
CHERNA
WIGAL-WOLLUMBUL
YUALI
BIGAM-BUL
KADAMBUL
KUDA-BAN
PAIKAL-YUG
SEE-EN-YUN
MARBUL
GINNIEBAL
WOL-ROI
KOOMIL-ROI
YOOKUM-BUL
KNITEN-BUL
BUNDEL
YUMPEIN
KOMBARANGA
HACI-GARI
BOORKUTTI
BANG-GETTI
GIROMBUL
INUMOH
KAMIL-ROI
KOINBERRI
HONNARUA
WORIMI
NYABA-KAL
DARWINUNG
GLAWE-GAL
CAMMER-RAY-GAL
DUARI-RUK
GWEA-GAL
WANDANDIAN
Bass Strait
Point Danger
Moreton Bay
Fraser I.
Point Stephens
Port Macquarie
Camden Haven
Jervis Bay
Twofold Bay
Disaster Bay
Mallacoota Inlet
Western Port
Point Nepean
Corner Inlet
Wilson's Promontory
Waratah Bay
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P
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150°
154°
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